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VOL. LXXX, JULY—DECEMBER, 1879.



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THE
RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND
PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M.D.

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN
AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

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SYNOPSIS.

AN ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, WITH OTHER SHORT ARTICLES FROM THE MEDICAL JOURNALS, SHOWING THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS DURING THE HALF-YEAR. ARRANGED ALPHABETICALLY.

AFFECTIONS OF THE SYSTEM GENERALLY.

AGUE.—*Quinetum Sulphate*.—Dr. Ainslie Hollis confirms Dr. Vinkhuyson's experience in regard to the good effects of quinetum in the treatment of ague. He finds that a dose of quinetum sulphate, of from ten to fifteen grains, is almost invariably sufficient of itself to stop the course of a febrile intermittent. In two cases out of twelve, in which it failed to do so, the subsequent administration of quinine was likewise without effect. Dr. Hollis therefore concludes that quinetum sulphate is a valuable anti-periodic remedy second only to quinine. The drug is scarcely half the price of quinine, and is therefore recommended to the favourable consideration of the medical staff of hospitals, and other charitable institutions, where economy is, or ought to be, particularly practised. Sulphate of quinetum is a white flocculent substance consisting of numerous fine acicular crystals, bitter in flavour, and readily soluble in acidulated water. It represents the whole of the alkaloids of the cinchona succirubra bark. It contains quinine sulphate, cinchonidin sulphate, and cinchonin sulphate in the proportions of 25·30, 50·55, and 20·25 per cent. respectively. The variations of the alkaloids rarely range to the extent of 5 per cent., a fact of some importance in estimating the therapeutic value of the drug. (Practitioner, June, p. 451.)

ACUTE RHEUMATISM.—*Salicin and Salicylic acid*.—Though salicin and salicylic acid do sometimes have an antipyretic action, their effect in this way is not marked, and is not to be compared to that of quinine. Salicin and salicylic acid are not antipyretic to any useful extent. They are anti-rheumatic; and their beneficial action in acute rheumatism is due, not to their allaying the fever, but to their putting a stop to the whole process of the disease, and to all that constitutes it—the fever as well as the other symptoms. As a rule, relief of

pain precedes fall of temperature. Salicin is prepared from the bark of different species of willow. The bark is removed in spring, when it contains the largest quantity of the bitter principle. It is a fact that salicylic acid and salicylate of soda not unfrequently give rise to considerable and even alarming depression. Such an untoward effect is not produced by salicin. From a therapeutic point of view this is one of the most important points of difference between the two remedies. In a disease, such as acute rheumatism, in which the heart is apt to be involved, the absence of this tendency to cause depression points out salicin as a much safer remedy than salicylic acid. Of the depressing action of salicylic acid many instances are recorded. Several have come under my own notice. The following is of value as the unbiassed evidence of an intelligent, well-informed medical man, founded on his experience of the two drugs. Dr. Sinclair, physician to the Dundee Infirmary, suffered from an attack of subacute rheumatism last December. Before I saw him he had been taking salicylate of soda in twenty-grain doses with relief to the pain; but it so depressed him, and made him feel so wretched, that he said he could not go on with it. I recommended salicin instead. He took it in even larger doses than the salicylate, with speedy relief to his rheumatism and without any untoward effect. On the contrary, he seemed, under its influence, to regain strength and appetite, and was soon quite well. The following is his own statement, given with his permission:—"Both drugs relieved the pain, tenderness, and swelling, when taken in full doses frequently repeated. But the salicylate, which I employed first, produced some very unpleasant effects. The taste I found to be disagreeably sweet and nauseous. After taking several twenty-grain doses, a copious perspiration was produced; the strength of the pulse was very distinctly diminished, while its frequency was increased; and a feeling of most uncomfortable depression, with singing in the ears, ensued. Indeed, I hardly knew whether the disease or the remedy was preferable. Salicin, on the other hand, has a pleasantly bitter taste; it improved the tone of my pulse and digestion, and relieved the pains more rapidly. Neither drug gave any relief except when taken in twenty or thirty-grain doses every hour for from six to twelve consecutive hours. It may be said that, had I taken smaller or less frequently repeated doses of the salicylate, I might have escaped all the disagreeable effects except the taste—itself no small matter. But such doses produced no effect on my rheumatism. To my mind one of the great merits of salicin is the absolute safety with which large doses can be taken.

In the course of one period of twenty-four hours I swallowed an ounce of it with nothing but benefit." (Dr. T. J. Maclagan, p. 41.)

CANCER.—*Local Treatment*.—That the removal of cancerous growths from the breast by *caustic* is much safer than that effected by the knife, was satisfactorily proved by the extensive trial of this method some years ago at the Middlesex Hospital. The great objection to the treatment was the extreme and long enduring pain which it caused, but this (as I had afterwards the opportunity of showing at the same hospital) can be completely prevented by combining congelation with the caustic. By freezing a tumour for more than an hour before applying chloride of zinc, the benumbing produced was so complete and lasting as to prevent pain during the whole time required by repeated cauterisation for its removal. The details of the process are given in a tract by the writer, published soon afterwards, and in Mr. Collis's work on Cancer. The principal objection to *pressure* as a remedy of cancer has been the difficulty of applying it and of regulating its degree from time to time, according to the feelings of the patient. For the pressure of a bandage, employed by Young and Récamier, Dr. Neil Arnott substituted, and with great advantage, the equal and more easily controllable pressure made by a spring and a slack cushion of air. Its effect, according to Dr. Walshe, was "the total removal of the morbid production in the most favourable cases," and in others "the gradual reduction of bulky masses to small, hard, flat patches or rounded nodules, which appear to be, both locally and generally, perfectly innocuous." It has been doubted whether the patches or nodules following the pressure are so innocuous as they have been represented, but the probability is that they are as inert as a bullet often is when bedded in the flesh, or as a permanent hard and bulky cicatrix. The following passage in Mr. Spencer Wells's work on Cancer bears on this question. "I saw," he says, "a lady a few weeks ago with a small tumour like a walnut in her breast, quite painless and not causing the least anxiety, which I saw nine years ago under the the compression treatment; and it was then to all appearance, a large cancerous tumour on the point of ulcerating. I have not the least doubt that if the tumour in that case had been removed either by the knife or by caustic, the patient would have been dead seven or eight years ago." The benumbing effect of *congelation*, so valuable when it is employed as an anæsthetic in operations, constitutes a powerful alleviative of suffering in external cancer. It has several advantages over the administration of opium. The action of

congelation is confined to the diseased part, whereas the persistent use of large doses of opium greatly disturbs the functions of the brain and digestive organs. Many patients would rather endure considerable pain than the stupefaction and sickness produced by opium. The relief from intense pain by opium is not so complete as that from congelation, and is far less durable. M. Broca, in his treatise just mentioned, adverts particularly to this long cessation of pain, inferring from it that some other organic change is produced by congelation than that of the vessels and nerves. He concludes his observations on the subject by the following statement:—“After the facts which have come to my knowledge, after those which I myself have observed, I am able to affirm that in many cases this method is a powerful alleviative of the pain which complicates cancerous and other tumours. It is a resource little used in France, but one which to me appears very valuable.” That congelation has been little used in England for this purpose is to be attributed to the difficulty of applying it in certain cases, especially when the uterus is the seat of the disease. The directions for this in the writer’s treatise on Cancer have not always been attended to, and means have been employed quite incapable of effecting what is required. Amongst others, ether spray has been resorted to, which can only act as an anæsthetic when its evaporation is unimpeded. When a wide speculum can be introduced, permitting the application of a strong freezing mixture to the morbid part for a sufficient period, there will seldom be a failure; but when in the advanced stage of the disease this cannot be done, relief may still be obtained from a powerful frigorific solution passing as a stream through a double catheter of small size; and if the outer part of this catheter be covered by an inflatable membranous sheath, the solution will be confined to the inner part of the canal. In congealing cancer of the breast or other outward parts, a small net of gauze or tulle may be used, containing the mixture; or, what answers much better, this may be applied by means of a gutta-percha cup fitted to the part. By the cup the smarting which might otherwise be complained of is in a great measure prevented, as the temperature can be very gradually reduced; and, if the cup is deep, pressure of any desired amount can be simultaneously made, either by the quantity of the mixture in the cup or by distending a bladder inserted in its upper part. (Dr. James Arnott, p. 49.)

SCARLATINA AND PUERPERAL FEVER.—Dr. Braxton Hicks has called attention to the rose rashes occurring in puerperal fever, and has connected them with sources of infection from

scarlatinal cases, &c. But these cases nearly all terminated in the ordinary symptoms of puerperal fever, and the object of this very important paper was mainly to show how ordinary puerperal fever arose from sources of infection of zymotic origin. Thus, he has proved that not only the contagium of scarlatina, but also those of measles, diphtheria, erysipelas, typhus, and typhoid, gave rise to puerperal fever. Nevertheless, if his cases are looked into carefully, it will be seen that only in those in which there is proof of the existence of the scarlatinal poison, has the definite rose-rash preceded the puerperal fever. This proof of the presence of scarlatinal poison was not always obtained prior to the outbreak of the puerperal fever, but in nearly all those cases in which it was difficult to prove its presence *before*, some indication of its existence *after* was afforded by an outbreak of the disease either in the children, or in some other persons living under the same roof. (Mr. H. G. Howse, p. 21.)

Surgical Scarlatina.—For the future any case which develops in our surgical wards a higher temperature with roseoloid rash either within a short period after an operation, or during the course of some surgical malady involving suppuration or discharges of any kind, ought to be treated by isolation, just as if it was a proved case of scarlatina, and not permitted to return to the ward until the period of desquamation is quite passed. (Mr. H. G. Howse, p. 25.)

SCARLET FEVER AND DIPHTHERIA.—*Salicylic Acid.*—Mr. Pownall affirms that salicylic acid is one of the most reliable remedies in the treatment of scarlet fever and diphtheria. He has used with unvarying success for the past three years salicylic acid suspended in mucilage, in both mild and severe forms of scarlet fever, and has seen the throat symptoms and fever speedily abate, and the patients make rapid recoveries. On being called to a case, he administers doses varying from five to ten grains every two hours until the throat symptoms and fever abate. He then finds that children, for whom so little can be done, when the mop or brush has to be applied to the throat, experience no inconvenience in taking this medicine, which, being simply in a state of suspension, has a chance—at least some portion of it—of remaining on the throat and so acting as a topical remedy, whilst the remainder acts as an invaluable anti-pyretic. The success of Mr. Pownall in cases of scarlet fever has led him to try the same remedy for diphtheria, and he has seen the pellicle broken up and the diphtheritic patch removed in the most marvellous manner. Indeed, since the use of salicylic acid in diphtheria, he states that he has not seen one fatal case, although several were of

a very dangerous type. In diphtheria the mode of action is to give the salicylic acid every four hours, and tinctura perchloridi (P. B.) alternately with it. Trusting solely to the salicylic acid, it was found in mild cases to answer every purpose, whilst in more severe cases, accompanied with much debility, there seems to be a tendency for the disease to return on discontinuing the remedy. The iron has been therefore added alternately with the acid as a blood restorer. To prove that iron was not the sole active agent in the cure, Mr. Pownall points to the many failures of iron as a local application in the past treatment of diphtheria, whereas with the salicylic acid treatment the pellicle has not spread under its use in one single case. The form used is \mathcal{R} . Acidi salicylici \mathfrak{z} i. vel. \mathfrak{z} ii., syrupi simplicis \mathfrak{z} iv., mucilaginis tragacanthi \mathfrak{z} i., tinctura aurantii \mathfrak{z} iv., aquæ q. sal. \mathfrak{z} vi., fiat mistura, capiat \mathfrak{z} iv. 2ndis horis. (Practitioner, Feb., p. 121.)

Salicylic Acid as a Prophylactic against Scarlatina, Measles, Diphtheria, &c.—Dr. P. C. Barker has, for more than five years, used salicylic acid as a prophylactic against the above diseases, and with very gratifying results, excepting in two instances, in the first of which the salicylate of soda was ordered instead of the acid, while, in the second, the acid was taken very irregularly. He has never had a second case of either scarlatina or measles in the same house, or among those known to have been exposed to infection, since beginning his experiments with this remedy. Some of the examples he quotes seem to show very strikingly the protective power of this agent. Children are stated to have lived and played in the same room—sometimes to have slept in the same bed—with the patient after the disease had unequivocally declared itself, and yet to have escaped entirely while taking salicylic acid. Dr. B. gives it in doses of one to five grains (according to the age of the person under treatment) once or twice daily, the single daily dose being sufficient when treatment is begun soon after known exposure to infection. This is continued as long as circumstances demand it, *e.g.*, where one of the same family is the invalid, it is kept up till convalescence is established and the house has been fumigated. Dr. B. also uses salicylic acid in diphtheria and typhoid fever to prevent the self-poisoning which so many cases, of the former especially, are apt to present. In diphtheria he orders a strong preparation, a solution of \mathfrak{z} i. in \mathfrak{z} ii. of alcohol, to which \mathfrak{z} vi. of hot glycerine or water are added; this is dropped on the dorsum of the tongue at half-hour intervals in severe cases. By this means he succeeds in abolishing foetor, and has repeatedly noticed an improvement in the appearance of

the throat soon after commencing its use. In typhoid fever he administers two to five grains every six hours, in capsule, as long as the exacerbation lasts. (New York Medical Journal; Glasgow Medical Journal, April, p. 333.)

SALICYLIC ACID.—*Uses of.*—There are two independent effects of salicylic acid: the germicide and the antipyretic; there are many conditions of disease where it would be well to make use of both these actions, and some where the antipyretic is distinctly aided by the germicide effects of the acid, so that fever is lowered more certainly and quickly by its use than when the more easily administered soluble salt is prescribed. This is well seen in *scarlatina anginosa*, and sometimes in *diphtheria*, whether the acid be conveyed to the throat directly, or be suspended in mucilage, or by means of glycerine, its most convenient solvent. Half an ounce of glycerine, when hot, will dissolve half a drachm of salicylic acid. This is stronger than necessary, and, when cold, will either deposit some of the acid or may become solid; in either case, it will redissolve when heated, and can be mixed in a warm spoon with an equal quantity of hot water, and given in small quantities with or without any drink afterwards; or, a solution of five grains of salicylic acid to the drachm of glycerine can be used, either alone or given with a little cream. In this way, not only are the mouth and throat cleansed, but the fever is soon lessened; it is only while the fever is high that the strong doses need be continued. In cases of moderate severity, it suffices to prescribe this weaker glycerine solution, and to order half a drachm or a drachm to be mixed with an ounce of water at the time of administration. The latter is quite strong enough for an adult, and is better followed by a drink of water. Or half an ounce of the glycerine in half a pint of water forms a suitable mixture; this sipped frequently or given as a drink every two or three hours, diminishes fever and improves the throat. Such a solution of two grains to the ounce is efficient as an antiseptic, and can be used in spray. Where a general antipyretic effect is desired, salicylate of soda may be given at the same time, fifteen grains being equivalent for this purpose to ten grains of the acid. It is contra-indicated where there is renal congestion or any albuminuria, as most of the acid is secreted by the kidneys. This method of administration is more suitable to scarlet fever than to diphtheria, where the necessity for giving iron restricts the use of salicylic acid to the intervals when the stronger form can be applied in small quantities frequently. In erysipelas, no form of salicylic acid is advisable; not only would it interfere with the use of iron, which is then essential, but

there is no febrile condition over which it has so little control as erysipelas. In *typhoid fever*, the use of salicylic acid presents some advantages over that of salicylate of soda. The glycerine solution is suitable for administration in *diabetes*, salicylic acid having a power of checking the formation of sugar not possessed by salicylate of soda. For this purpose the acid is required in full doses; it might take the place of carbolic acid in rendering diabetics more tolerant of operations and less liable to suffer from boils and from suppuration. In *catarrhal sore-throat*, or at the commencement of a common cold, the weak solution of salicylic acid is beneficial. For checking the febrile reactions in *phthisis* it is also preferable. It also acts as a sedative to the pneumogastric, and the weaker glycerine solution in water relieves cough. As a remedy in *whooping-cough*, this solution may be found as effective and more convenient than the laryngeal insufflation of the powder. *Hay-fever* is checked by dropping a grain to the ounce solution into the nares. The great obstacle to the freer use of salicylic acid is its sparing solubility in water; this difficulty has been overrated. Solutions of one or two grains to the ounce keep clear or deposit a few flocculi only, when theoretically all but one-fifteenth of a grain should separate. (Dr. W. Squire, Brit. Med. Jour., April 26, p. 625.)

STIMULANTS.—*Action and Uses of Alcohol in Health and in Disease.*—Stimulants seldom benefit, and often do harm, when taken alone, even in moderation. They are of use only at meals, and especially at dinner. 2. They ought not to be taken till that period of the day when business is over, and relaxation or amusement begins. In particular, very few people can take stimulants at dinner if they have afterwards to transact much serious business of a sedentary nature; if they use them they must have an after-dinner sleep before they are fit for work. 3. Stimulants answer better, on the whole, under bodily than under mental exercise. They answer peculiarly well with those who betake themselves for a season to country sports and other brisk exercises, as a restorative after long exercise of the mind in a sedentary profession. But still in this case they ought not to be used till exercise is over for the day. Taken in moderation after vigorous exercise, they remove fatigue and aid digestion; but their action is followed by a stage of languor, which is unfavourable to the continuance of exercise. 4. It is a great error to use stimulants, during active bodily exercise, at frequent intervals, apart from meals. There is no doubt that in this way, by relieving fatigue, artificial strength is gained for a time; but in a long day's exertion the ultimate result is

unfavourable: more exhaustion is felt at the close, and the sense of fatigue next morning is greater. At the same time, no question can arise, that, under brisk exercise, stimulants taken in moderation, or even somewhat in excess, occasion much less injury than when used to the same extent during a sedentary life. Secondly. Of alcoholic liquors in the treatment of diseases. All diseases, in reference to the use of alcoholic liquors in their cure, may be aptly considered as of two great classes—diseases of reaction and diseases of exhaustion. In the former, the circulation being in too active a state, they are generally hurtful; in the latter, the circulation being enfeebled, they are commonly beneficial. In a state of reaction they strengthen and quicken the pulse, augment local congestion, direct the blood with force towards the head, and aggravate nervous derangement. In a state of exhaustion, on the contrary, they exert a tonic action, strengthening the pulse without accelerating it, adding to the muscular power, removing faintness, giddiness, and sickness, and subduing the multifarious nervous symptoms which are apt to accompany great debility of the circulation. These general facts will for the most part form a trusty guide to the employment of stimulating liquors for the cure of diseases at large. Never be in haste to resort to stimulants in *fever*. It is a very ill omen when a skilful and cautious physician finds himself reduced to the necessity of allowing wine before the first seven days are over. Another, and an imperative, rule in all cases is to watch diligently the effects of wine for some hours after it is first administered. We may be satisfied that it is to answer when the pulse becomes fuller, but not jarring or more frequent; when the attitude, eye, and general expression denote less languor or less stupor; when the tongue becomes more clean and moist at the edge—perhaps the most unequivocal of all signs that we are on the right track; when delirium abates, or sleep takes the place of rambling; when the patient is more easily roused, and to a state of improved intelligence; when the appetite begins to return. On the contrary, stimulants should be withdrawn if the face become more flushed, the delirium or the stupor more intense, the pulse more jarring or more frequent, the tongue more dry, foul, and retracted, and the restlessness and jactitation more distressing. (Sir Robert Christison, *Medical Times and Gazette*, Nov. 30, 1878, p. 623.)

Alcohol is a most valuable therapeutic agent in both typhus and typhoid fever. A large percentage of cases not only do not require it, but its administration is apt to lead to complications. It is impossible to lay down rules as to the stage of

the fever in which it may be indicated, as this indication depends rather on the type of the fever than on its stage. But the time to watch for its administration in ordinary cases is from the eighth to the twelfth day. Early administration of stimulants in fever is injudicious. He had little faith in their early employment *preventing* an adynamic condition, and he had rarely seen them have a good effect in the early stages of the fever of habitual drinkers. He was inclined to think that it is a dangerous fallacy to regard them as essential in such cases. Of the two, he had more often seen hard-drinkers recover without stimulants than with them. Alcohol, in his experience, had little effect on the temperature of fever. He generally took, as his tests, the age of patients, the condition of the heart, the pulse, tongue, and the head symptoms. Young patients, as a rule, do well without stimulants. A feeble, irregularly acting heart, with weakened first sound; a compressible and rapid pulse; a tongue keeping fairly moist; the absence of violent head-symptoms; encouraged him in their continuance and use. He believed that we possess in alcohol a supporting food in those typhoid states when assimilation is difficult, and he had many times seen lives saved in fever, sustained for days by brandy and milk alone, when everything else had been rejected. The practices he conceived most to be deplored in the administration of alcohol were: its indiscriminate employment in the earlier stages of fever; the rash continuance and the increase of the quantity used, when the symptoms show that it is acting injuriously and is of no service. (Dr. H. Macnaughton Jones, p. 9.)

TYPHOID FEVER.—The theory of *contagium vivum* is highly dangerous in the hygienic point of view, since it practically excludes the possibility of diseases such as typhoid fever being generated *de novo*, for the only alternative would compel us to admit the unphilosophical doctrine of heterogenesis. Let it be once admitted that such poison can be produced by fæcal decomposition, and that such decomposition can occur within our own dwellings; and let it be moreover acknowledged that, just as in the case of the septic poison, decomposing matter is deadly, while that already putrid is rather offensive to our senses than toxically dangerous, and we shall hear much more of house-drains, and far less of sewer-gas. I do not wish, however, to convey to you the impression that I consider healthy fæcal matter capable of evolving the typhoid poison during the process of its decomposition. Were it so, I fear the disease, unhappily prevalent as it is, would be far more common. Healthy stools, like normal urine, are

non-albuminous, while in certain diseased conditions either the one or the other may contain albumen. The evacuations of patients suffering from intestinal inflammation or ulceration—such, for example, as we meet with in typhoid fever or tubercular disease—are always albuminous: and possibly it is by the splitting up of this highly complex compound that the poison is generated. If we can divest ourselves of the notion that the typhoid poison is a living entity propagating its kind by one or other of the various methods peculiar to vitalised bodies, I think there is no great difficulty in assuming the possibility of its arising *de novo*. So far as we are aware, the principal morbid change which takes place in the body of a person affected with this disease consists in a species of inflammatory affection of the intestinal mucous membrane, specially attacking the glandular structures known as Peyer's patches. Moreover, the evidence is conclusive that the evacuations from this diseased surface contain the great bulk of the poison generated in the body of the patient. I do not believe that the typhoid poison can result from the decomposition of healthy stools; but when those stools cease to be healthy, and become albuminous, I think it is by no means improbable that such may be the case. (Dr. R. King, p. 3.)

AFFECTIONS OF THE NERVOUS SYSTEM.

CHLORAL POISONING.—*Nitrite of Amyl*.—When chloroform is administered in excess *too rapidly*, it seems to prove fatal by paralysing the respiratory centres, while the pulse remains comparatively unaffected, the pupils being *contracted*; but when chloroform inhalation is kept up *too long*, so that the drug accumulates slowly in the system, the heart first yields to its influence, and succumbs earlier than the respiration, and under these circumstances the pupils will be found *dilated*. I have ascertained these conditions, both experimentally in the lower animals, and from a large experience of chloroform administration commenced twenty-two years ago, while assistant to the late Sir James Simpson. Liebreich, the discoverer of chloral hydrate, believes that it acts on the system by being resolved into chloroform from decomposition in the presence of an alkali. This would at once explain why nitrite of amyl should be the best antidote in chloral-poisoning, much more certainly than strychnia, which has been proposed as its antagonist; while, strangely enough, nitrite of amyl itself is proposed by Dr. B. W. Richardson as the antidote to strychnia poisoning. May it not be that nitrite of amyl will prove the appropriate antidote when the drug has been administered in

such quantity as to act rapidly on the respiratory centres, *with contracted pupils*, and that strychnia should be given when the drug has acted slowly as a cumulative poison when the heart has succumbed, *and the pupils are found dilated?* (Dr. J. G. Sinclair Coghill, p. 295.)

CHOREA.—*Chloral Hydrate*.—In many cases drugs exert only a secondary influence, rest, warmth, and proper food being all that are required; but the class is by no means small in which iron, quinine, arsenic, phosphorus, and strychnia fail as remedial agents. Chloral hydrate has been recommended in large doses in violent chorea. The principle of treatment was to give thirty grains, and to repeat the dose, or half of it, if the patient did not obtain ten hours' sound sleep in the twenty-four. On waking, a second dose was given in proportion to the ascertained effect, but always less than the first. On waking again another dose less than the second, and so on till the amount of sleep had been obtained, when the chloral was discontinued till the next night. Of two patients so treated, aged eighteen and twenty, one was completely cured in one day, and the other on the fourth day. There can be no question whatever that hydrate of chloral is a valuable remedy in some cases of chorea, particularly in those where vascular excitement is present and the pulse is good. Dr. Althaus considers that the theory of chorea is explained by active hyperæmia of the corpora striata and the parts surrounding the fissure of Sylvius, and that the beneficial action of hydrate of chloral is to be attributed to the anæmia which it produces in these structures. Its danger as a depressant is nothing compared to the repose and rest which it ensures to the nervous system, lessening as it does in suitable doses the extreme agitation of the limbs, and the violence of the choreic movements. Sleep so obtained gives the necessary time for repair to the over-excited parts, and will be found to succeed when morphia yields no result. (Dr. W. H. Day, p. 56.)

Sulphate of Zinc.—With regard to sulphate of zinc, small doses are sometimes utterly useless, when large doses succeed; and if it is determined to try the remedy at all, it should not be set aside till the latter have had a fair trial. I have given this drug in doses of from one to five grains three times a day, and continued it for a week without producing any effects, and the remedy has so repeatedly disappointed me that for some time I ceased to employ it. This most likely arose from giving it in too small a dose. Sir T. Watson gave it successfully in ten-grain doses three times a day, in a severe case which had resisted other remedies.

There can be no doubt that zinc sometimes succeeds where iron and other remedies fail. In prescribing it, the dose should not exceed a grain three times a day to begin with, and should be gradually increased till there is nausea, or an amelioration of the symptoms. (Dr. W. H. Day, p. 58.)

DISCONTINUANCE OF OPIUM.—Patients who have been in the habit of taking opium in large doses for years are frequently seen at the Lambeth Infirmary, but Dr. Lloyd affirms that he has never known any evil results follow the practice of discontinuing the drug suddenly. “I always,” he says, “place them on a nourishing diet, without stimulants, and administer quinine in one or two grain doses.” (Dr. R. H. Lloyd, p. 297.)

FACIAL NEURALGIA.—*Aconitin*.—Prof. Gubler states that he has never met with a case of neuralgia of the fifth nerve, even tic douloureux, which failed to yield to aconitin. A man on whom Nelaton had performed the operation of resection of the nerve for violent neuralgia, with only temporary improvement, was entirely cured by aconitin, of which he took $\frac{1}{14}$ of a grain. Hottot's preparation of the nitrate of aconitin is the best to use; the dose is $\frac{1}{140}$ of a grain which contains $\frac{1}{280}$ of a grain of aconitin. When this quantity fails to produce the desired effect, the doses may be increased up to $\frac{1}{12}$ of a grain. Heart disease absolutely counter-indicates the drug. (Prof. Gubler, Practitioner, June, p. 457.)

Sulphate of Copper.—At a meeting of the Académie de Médecine in Paris, M. Féréol read a paper upon the good results to be obtained in cases of neuralgia of the fifth pair of nerves by the use of the ammoniacal solution of copper sulphate. Although the cases in which this method of treatment had been employed were not numerous, being but six in all, of which four had occurred in M. Féréol's own practice, they were still of importance, because the neuralgia was, in nearly every case, of long standing, had resisted all other therapeutic means, and presented the characters of facial tic douloureux. The disappearance of the pain was instantaneous, and although the painful phenomena might not have entirely vanished, still the improvement was very marked. The average dose of the medicine was 0·10—0·15 per day, which could be raised gradually to 0·30—0·50. It could be given in the form of pills, in which case it was found necessary to break up the dose into eight or ten pills, one of which was to be taken every hour or two, preferably at meals. The sulphate of copper should be continued for twelve or fifteen days at least to prevent any relapse. (M. Féréol, Practitioner, July, p. 48.)

NERVE AFFECTIONS.—*Action of Belladonna*.—All the effects of belladonna may be accounted for by supposing that it paralyzes the peripheral nerves belonging to the cerebro-spinal system, except those supplying voluntary muscles; while it has no effect upon the nerves derived from the sympathetic. It is often useful in toothache (less markedly, however, than gelsemin), and in rheumatic or gouty pain; while it has no effect upon the neuralgias of hysteria or other centric affections. Perhaps its effect upon epilepsy is due to its arresting the centripetal stimulus producing the fit. Whether this be so or not, it has been too much neglected since the use of the bromides; and sometimes at least, it has an effect when these fail; it may also be advantageously combined with them. Belladonna relaxes spasm of the involuntary muscles. Its effects on the eye belong to a subject which is too special for me to handle; its power of dilating the os uteri, and relieving tenesmus of the bladder and rectum when applied locally, is equally important, but perhaps less well known. A further example of this mode of its action is its influence on constipation, which Trousseau first made prominently known. This action upon involuntary muscles is frequently increased by the anæsthetic effect simultaneously exercised by belladonna upon the afferent branches of nerves which originate spasm, as in the cases of whooping-cough and spermatorrhœa. When added to a hypodermic injection of morphia, it often prevents the nausea and vomiting which this may produce; and when given with an opiate in phthisis, it not merely prevents sweating but appears to keep up the activity of the lungs and heart. (Mr. J. R. Gasquet, p. 307.)

NEURALGIC HEADACHE, ETC.—*Croton Chloral*.—I have used this medicine largely—sometimes failing, sometimes relieving—till, by keeping an account of all my cases, it began to dawn on me which were benefited by the drug. Since then the number of cases relieved (some permanently) has increased. These cases are—headache in females arising from mental distress; those cases of headache so frequent at the menopause—in fact, all those called neuralgic, except a few arising from internal mischief, are benefited, and, in many instances, cured. In that distressing species of neuralgia called tic-doloureux I have found it in many cases acting like a charm. Of course I do not include any arising from cranial or intracranial causes. I have tried it in neuralgia of the ovaries, but no good resulted. In insomnia it is not so reliable as the hydrate, but in some cases where the loss of, or inability to, sleep is accompanied by a weak or fatty heart, it is to be preferred, as it has no weakening effect on the central organ of the

circulation. In one case of delirium tremens, where the circulation was very feeble, the combination of croton chloral with digitalis had a wonderful effect, and it seemed as if the drugs could be given together in much smaller doses, to produce the same results, than singly. In this I pushed it from 10 to 30 grains every three hours, with drachm and 2-drachm doses of the infusion of digitalis. In pain arising from caries of teeth I have found it useless in most cases, and in all inferior to Richardson's "tr. gelsemini;" but in one case of a nervous young lady, by giving her two 10-grain doses I was able to extract a tooth next to painlessly, to her great satisfaction. You will notice in all these cases it is in affections of those parts supplied by the fifth pair of nerves that it is of most use; but, to be of service, you must give the drug in far larger doses than prescribed in the Pharmacopœia—for adults, 5 grains three or four times daily, gradually increasing if required; if stimulants are wanted, dissolve it in rectified spirit; if not, dissolve it in glycerine. In all cases complicated with hemorrhoids, give glycerine. If anæmia exists, combine it with iron, or, which I believe better, arsenic; then gradually lessen the chloral. In all cases I have found it better to give it in solution than in powder or pill. (Dr. R. Riddell, p, 291.)

OPIUM POISONING.—*Subcutaneous Injection of Atropine.*—A man thirty-five years of age had taken six drachms of tincture of opium an hour before admission. Vomiting could not be induced, stomach pump used after his drowsy state prevented his refusal to allow it. State of patient got worse and worse. About four hours after the opium had been taken, and when the symptoms of poisoning were worse than they had been, and when the patient was absolutely unconscious, and could not be roused, one-fourth of a grain of sulphate of atropine was injected. Within twenty minutes the respiration went up from 12 to 18, and the pupils began to dilate, and, except that respiration failed for a short time, the effect of the opium gradually passed away, and in seven hours more the patient awoke, and took some food. The change for the better took place almost immediately after the atropine was injected, and just when the evidence of its action on the pupils became manifest. (Dr. J. E. Eddison, p. 298.)

SCIATICA.—*Sulphuric Ether.*—Dr. Comegys states, in the Cincinnati Lancet, that in his hands, and in those of other practitioners, the hypodermic injection of 30 minims of sulphuric ether, night and morning, a little posterior to the great trochanter, has effected a cure in sciatica. Dr. Starr, of Philadelphia Episcopal Hospital, injects one-eightieth of a

grain of atropia into the tissues with manifest advantage. (Glasgow Medical Journal, June, p. 484.)

TETANUS.—*Subcutaneous Injection of Atropia.*—Tetanus, *i.e.*, a series of reflex phenomena depending upon an over-excited or congested state of the brain, the spinal cord, and their membranes, is capable of being relieved, or even cured, by atropia, when administered in comparatively small doses, extended over a certain period of time according to the severity of the symptoms; though we know from the experience and experiments of Drs. Harley, Fraser, and others, that when given to its full physiological effect it produces excitement and congestion of the cord, followed by the usual reflex results, as jactitation, muscular spasm, and convulsive fits. (Surgeon D. H. Cullimore, p. 59.)

TRIGEMINAL NEURALGIA.—*Gelseminum.*—In cases of neuralgia of the trigeminus, Dr. Massini gives twenty minims of the tincture every half hour up to three doses, and he finds that the first dose generally affords relief, and that the pain rapidly subsides after a second or third dose has been taken. He has never found it necessary to exceed sixty minims, and only in one case did this quantity produce unpleasant head symptoms. The cases in which the remedy produces most benefit are those of simple rheumatic neuralgia of the alveolar branches of the trigeminus; in those it rarely fails. It also sometimes relieves the pain remaining after the stoppage of a carious tooth. When there is any inflammatory affection of the bone or periosteum, no good can be expected from the remedy. The medicine may, if necessary, be repeated several days in succession, the active principle rapidly passing off by the kidneys. (Dr. Massini, Practitioner, July, p. 45.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

NÆVI.—*Bloodless Removal of.*—For many years I have been in the habit of resorting to excision in preference to the other modes of treating nævi; and I have found that in a large proportion of cases primary union follows, while in the instances in which this result is not attained recovery is, as a rule, more rapid, and the subsequent scar less visible, than when the ordinary methods of ligature or cautery are adopted. A baby, 10 weeks old, was brought to me with a large nævus on the back of the neck. It was nearly circular with a diameter of an inch, and an elevation of about half an inch. The greater part was subcutaneous. I first passed three strong hare-lip pins under the base of the tumour so as to cross one another about its centre, with their points of entrance

and exit at least a quarter of an inch outside its circumference; I then took an ordinary india-rubber drainage tube, and wound it round underneath the heads and points of the pins, until it was evident that the circulation in the nævus was completely arrested. It was now quite easy to dissect back the healthy skin, by which part of the vascular tissue was covered, and to remove the affected skin together with the subcutaneous portion of the growth. There was no difficulty in distinguishing the limits of the disease, and not a drop of blood was lost during the excision. On removing the pins, a sharp hemorrhage ensued, but this was readily controlled by pressure. Silver sutures were used to draw the edges together, and boracic acid lint was applied. In three days the sutures were taken out, and primary union was found to have followed along nearly the whole line of the wound. The rest healed rapidly, and a small linear cicatrix was all that remained to indicate the site of the tumour. The plan which I now use most frequently, is the following. I first take two strong needles threaded with wire sutures and transfix the base of the tumour, the needles lying parallel to each other, and perpendicular to the long axis of the nævus, with their points of entrance and exit at least a quarter of an inch from its margin. A hare-lip pin is next introduced in the same way but at right angles to the needles. After winding the india-rubber tube as before, beneath the exposed extremities of the needles and pin, I dissect out the growth, leaving the skin over it, if it seems but little affected. The next step is to draw the needles out by means of a forceps, so as to leave the wire sutures in their place. Then after bringing together the edges of the wound by twisting the wires, the tube and pin are withdrawn, a pad of boracic acid lint applied, and gentle pressure exerted by strapping and a bandage. No blood is lost during the operation, and after the dressing has been applied it is rare to find that so much hemorrhage occurs as to stain the surface of the pad, which consists of four or six thicknesses of lint. More bleeding than this I have never yet seen. (Mr. N. Davies-Colley, p. 159.)

FORCIPRESSURE, AND THE USE OF FORCEPS PRESSURE IN SURGERY.—In the removal of large ovarian and uterine tumours, amputation of the breast, and the removal of large tumours in other situations, some means of arresting hemorrhage during the progress of the operation is necessary. In many cases pressure-forceps have always been sufficient for the suppression of hemorrhage, at least until the completion of the operation. Any vessel divided in the first incision through the skin and integuments, either of the abdominal

wall or any other part of the body, may be seized almost as soon as divided, and the forceps left hanging on as long as necessary. Deeper vessels may be secured in the same way. I have removed very large mammary tumours, one weighing nine, and another thirteen pounds; sometimes putting the forceps on a large vessel before dividing it, sometimes directly after the division, until from twelve to twenty forceps had been used, and thus large vascular tumours were separated with scarcely any loss of blood. In ovariectomy, shreds of vascular omentum or mesentery may be thus secured before separating them from the cyst, or any vessels which bleed after separation from the cyst may be made safe until the pedicle has been secured and the tumour removed. If it be inconvenient to tie the pedicle before dividing it, then two or three pressure-forceps may be applied, one at each extremity of the pedicle, and one or two more towards the middle; and the tumour may be cut away without any fear of losing the end of the pedicle or its vessels. In Cæsarian section, splenectomy, nephrectomy, and the removal of a large undescended testicle in a man, forcipressure has proved quite as useful to me as in ovariectomy or hysterectomy. In operating for the cure of old ruptures of the perineum, troublesome bleeding often occurs as the mucous membrane is denuded. If ligatures be used, they may interfere with union between the raw surfaces when they are brought together by suture; but, as a rule, pressure-forceps amply suffice for stopping all bleeding until the sutures have been passed and are ready for closing. A little pressure is then all that is needed. So in operating on vaginal fistulæ, very troublesome bleeding, which would interfere with accurate paring of the edges and passing of the sutures may be immediately and effectually controlled; and if the forceps be left on until it is time to fasten the sutures, it will almost always be found that bleeding has ceased. (Mr. T. Spencer Wells, p. 148.)

THE SPHYGMOPHONE.—*An Instrument for making the movements of the Pulse audible.*—While experimenting with the audiometer, it occurred to me that I might get a secondary or telephonic sound from the movements of the pulse at the wrist. I have effected this in a very simple manner, by adding a microphone to a POND'S sphygmograph. I mount on a slip of talc, glass, wood, or ebonite, a small plate of metal, such as platinum, or a little bar of gas carbon. I place the slip in the sphygmograph as if about to take a tracing of the pulse. I connect one terminal from a Leclanché's battery to the metal or carbon, and the second terminal from the cell to a terminal of the telephone. Then I connect the other

terminal of the telephone with the metal rod of the sphygmograph which supports the slip. The instrument is now ready for use. It is placed on the pulse in the ordinary way, and is adjusted, with the writing needle thrown back, until a good pulsating movement of the needle is secured. When the movement is in full action, the needle is thrown over to touch the metal or carbon plate, which it traverses with each pulse-movement, and completes the connection between the telephone and the battery. The needle, in passing over the plate, causes a distinct series of sounds from the telephone, which correspond with the movements of the pulse. When all is neatly adjusted, the sounds heard are three in number, one long and two short, corresponding to the systolic push, the arterial recoil, and the valvular check. The sounds are singular, as resembling the two words, "bother it." The sounds can be made very loud by increasing the battery power. (Dr. B. W. Richardson, p. 72.)

TORSION OF ARTERIES.—As to the means best adapted for securing vessels and preventing the recurrence of hemorrhage, I cannot speak too strongly in favour of torsion in preference to ligature. Indeed, I think the time has come when the silk ligature ought to be regarded as a thing belonging to a past age, and as much out of date as bleeding in fevers, or the administration of mercury for gonorrhœa. I am convinced that timidity on the part of the surgeon is the chief thing that stands in the way of the universal practice of torsion. (Mr. R. Clement Lucas, Guy's Hospital Reports, p. 325.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

ANGINA PECTORIS.—*Preparations of Nitro-Glycerine.*—Considerable interest has recently been attracted to this drug by the discovery of its power of checking the paroxysms of angina pectoris. In its action on the bloodvessels it is allied to nitrite of amyl; and it is found that a drop or two of a one per cent. alcoholic solution, given in a little water during a paroxysm, will afford almost instant relief. For patients of sedentary habits, this solution, which is perfectly tasteless, is undoubtedly the most convenient preparation; while for those who are much away from home, Mr. Martindale, of New Cavendish Street, has succeeded in putting the drug into the form of pills and lozenges, or "tablets." The pills can be made of any strength; but the dose usually given is a hundredth or a fiftieth of a grain every four hours, an extra dose being taken immediately the pain is felt. They are

almost inactive when swallowed whole, but speedily afford relief if chewed in the mouth. The pills have a slight sweet chocolate flavour. In a case of angina pectoris in which they were prescribed, the relief afforded was most marked. The lozenges, which are evidently made from chocolate-paste, are of three sizes, corresponding to one, two, and three minims of the one per cent. solution. They are certainly active; even the smallest producing the full physiological action of the drug, whilst they are agreeable to the taste. It is no easy matter to obtain nitro-glycerine in combination with a fatty substance, and Mr. Martindale is to be congratulated on the way in which he has overcome the difficulty. (British Medical Journal, June 14, p. 899.)

ASTHMA.—*Drug Smoking.*—Of the various ways in which medicines may be conveyed into the lungs,—inhalation by steam or atomised vapours, fumigation with powders, and smoking, the last mentioned is the best. In his experiments Dr. Thompson used cigarettes made according to the following formula:—Swedish filtering paper, 4 in. \times $2\frac{1}{2}$ in.; potas. nitratis, gr. $\frac{1}{4}$; tinct. tabaci, ℥ x; ol. anisi, ℥ $\frac{1}{8}$. The paper is impregnated with the other ingredients, and then dried; the anise and tincture of tobacco (the latter made with $2\frac{1}{2}$ oz. of the leaves to a pint of spirit) disguise the odour of the burning paper, while the nitre causes the cigarette to burn continuously. A solution of the drug to be experimented upon having been prepared, the paper is floated through it, dried, and cut into a certain size, so that the dose given is accurately measured. The paper is then rolled as tightly as possible into the form of a cigarette, and smoked in the ordinary way. If the full effect of the dose be desired, the patient should be instructed to expand the chest with a full inspiration, and to retain the smoke in the lungs. It was found that, used in the ordinary fashion, *i.e.*, rejecting a considerable part of the smoke, a cigarette containing $\frac{1}{64}$ of a grain of extract of opium produced decided dizziness in the course of a few minutes, when smoked by healthy men; as at least half of the drug is in this way lost, it seems a fair inference that $\frac{1}{128}$ of a grain of extract of opium can exercise a marked influence on the system when introduced through the lungs. Several cases of harassing night cough (due to phthisis, asthma, mitral obstruction, &c.) are then given, in which the opium cigarettes had a very good effect, the cough being relieved and calm refreshing sleep obtained. The smoking of opium is especially adapted for cases of night cough, of laryngeal ulceration in which deglutition is painful, and of asthma. For soothing and diminishing the

dyspnœa, neurotics may be used with great effect; and the following combination has given the best results. The same form of cigarette is used as described above, and the paper is soaked in the following drugs according to the recipe here given:—*Rx.* Extract of opium, gr. $\frac{1}{64}$; extract of stramonium, gr. $\frac{1}{32}$; tincture of Indian hemp, $\mathfrak{M} \frac{1}{2}$; tincture of hemlock, $\mathfrak{M} 1\frac{3}{4}$; tincture of lobelia, $\mathfrak{M} 1\frac{3}{4}$; tincture of tobacco, $\mathfrak{M} 9$; oil of anise, $\mathfrak{M} \frac{1}{8}$; nitre, gr. $\frac{1}{4}$. Or, for a sheet of Swedish paper sufficient to make sixty-four cigarettes the formula may be given thus:—*Rx.* Tincturæ tabaci, 3x; tincturæ conii, 3ij; tincturæ lobeliæ, 3ij.; tincturæ cannabis Ind., $\mathfrak{M} \text{xxxij}$; extract. opii, gr. i.; extract. stramonii, gr. ij; olei anisi, $\mathfrak{M} \text{viiij}$; potassæ nitratis, grs. xvi; spirit. v. r. ad. $\mathfrak{Z} \text{ii. ss.}$ This formula, which is a complex one, has only been obtained from repeated experiments, leading step by step to the addition of some effective remedy, and to the elimination of less effectual drugs. As it is sometimes desirable not to give opium or Indian hemp, I have had cigarettes made with stramonium and lobelium only; so that altogether I have three different kinds for use: opium cigarettes, containing a small quantity of opium and stramonium; a compound opiated cigarette containing the drugs given in the formula above, and a stramonium cigarette without opium. Mr. Rowe (of the firm of Strickland and Rowe, Cromwell Place, South Kensington) can supply these cigarettes. (Dr. R. E. Thompson, p. 93, and vol. 78, p. 355.)

Hypodermic Injection of Morphia.—Dr. Huchard in L'Union Médicale, strongly advocates the use of subcutaneous injections of morphia in cases of asthma. Having found that it causes marked relief in the spasmodic efforts at respiration, he argues that it exerts a special influence on the respiratory system. On this hypothesis he pursued its application in other affections where the difficulty of respiration becomes a troublesome symptom, namely, in aortic disease, accompanied with cerebral anæmia, phthisis, and uræmia. Four well-marked cases of relief of laboured inspiration, associated with intestinal nephritis, are carefully detailed. Dr. Huchard thinks that the hypodermic injection of morphia cannot be replaced by opium preparations or by morphia introduced into the alimentary canal. When carefully injected, the morphia causes no serious inconvenience, 0·003 to 0·005 grains are first used, and the dose is gradually increased to 0·01 or 0·015 grains at the most for each paroxysm. Morphia relieves dyspnœa not so much by its hypnotic action as by its direct action on respiration, for clinical experience establishes the fact that it facilitates respiration. By the use of morphia,

the pain is relieved, the general system is thereby benefited, and the disease is thus probably modified. In a word, "morphia makes one breathe freely." (Dr. H. Huchard, Practitioner, July, p. 53.)

DIPHTHERIA.—Dr. G. Law, U.S.A., having been attacked by diphtheria, adopted the following line of treatment:—He swabbed the throat every three hours with undiluted tincture of the muriate of iron, and took internally, at similar intervals, ʒ ii of citric acid in iced water sweetened liberally. At the end of two days, during which time he had taken over three ounces of the acid, the membrane was coming away in soft flakes, and convalescence, in other respects, was established; the urine, also, was very copious and alkaline. (Glasgow Medical Journal, April, p. 330.)

HOOPING COUGH.—*Tincture of Myrrh*.—Thirteen illustrative cases, selected from a much larger number, are related by Dr. Campardon in the Bull. Générale de Therap., and in these the treatment seems to have been markedly successful. The ages of the patients varied from two to fifty-three years, and all presented the characteristic paroxysmal cough. Tincture of myrrh, in doses of 3-15 drops every hour or two hours, in a small quantity of cinchona wine, was administered, with the result of cutting short the disease in three to eight days—that is, the spasmodic element was completely eliminated from the cough, and in those in whom this constituted the whole of the disease recovery was complete within the time stated; catarrhal affections of the respiratory organs, however, had to be combatted in the usual way. Dr. C. concludes that hooping-cough yields easily and rapidly to the administration of tincture of myrrh, in vin de quinquina as a vehicle; the latter is by no means essential, but it certainly aids the action of the former. This treatment is not incompatible with the simultaneous adoption, when necessary, of such measures as are appropriate to the relief of the tracheo-bronchitis or pulmonary congestion so often seen in this affection. (Glasgow Medical Journal, April, p. 334.)

LOCAL EMPYEMA.—If under any circumstances whatever of an inflammatory attack in the chest, under whatever name that attack may be called, there result localised dulness, with absence of breath-sound, and perhaps distant tubular breathing, an empyema may be safely suspected. This is an opinion which I have reached from the very uniform termination of a number of disputed cases, where these physical signs have existed. I mean this: let a patient have an undoubted acute pleurisy, and if, after the urgent symptoms have passed, there still remain the above physical signs in one part of the chest,

some fluid may be considered to be left, and this is pus. If the case be one of pleuropneumonia, and there result after the acute attack the same local physical signs; then also an empyema is present; and more than this, if it have been thought from the nature of the attack that the inflammation was confined to the lung alone, and yet, after some time, when all other symptoms have ceased, there remain a localised dulness, with absence of lung breath-sound, it implies that a pleurisy had accompanied or succeeded the pneumonia, and, as a result, a local empyema is present. Thus, under all circumstances, with an history of an inflammatory attack, the result and the conclusion are the same. (Dr. S. Wilks, p. 77.)

NIGHT-SWEATS OF PHTHISIS.—*Belladonna*.—We cannot too frequently direct the attention of the profession to the invaluable action of atropia or belladonna in night sweats, so prevalent and prostrating a symptom in cases of debility and of consumption. We have used it in the form of powders of atropia, $\frac{1}{60}$ grain with sugar, or $\frac{1}{4}$ — $\frac{1}{2}$ grain solid extract in the form of pills, or 10—20 minims of the tincture, which is found to answer equally well. The following formula is almost a specific in phthysical cases with troublesome coughing:—*Rx.* Atropia sulph. grs. i., morphiæ sulph. grs. viij., acid sulph. aromat. 3 ij., aquæ menth. pip. ad. 3 i. *M.* dose, 5 drops thrice daily and at bedtime. The morphine and acid may be omitted, and in sore throat, giving rise to coughing, nitric acid may be substituted for the sulphuric acid with advantage in five-drop doses. The tincture is being used with good results externally in night-sweating. (Canada Lancet; Practitioner, May, p. 376.)

When atropia is used for the purpose of checking the profuse night-sweats of phthisis, it should be given hypodermically, in doses of from $\frac{1}{200}$ th to $\frac{1}{100}$ th of a grain. The injection of atropia has been used with success where oxide of zinc, gallic acid, and other drugs have been tried in vain. The gr. $\frac{1}{100}$ will often succeed where gr. $\frac{1}{200}$ has failed. One injection may completely stop the perspiration, and although the patient remains under observation for some weeks, there is no complaint of its return. These cases are not common, but it not unfrequently happens that after a few injections the perspiration, although not completely stopped, is checked to such an extent as to render further treatment unnecessary. An injection of atropia will often relieve cough when in excess of the amount of expectoration, and thus enable the patient to obtain a good night's rest; but it is not only in this way that the perspiration is checked, for night-sweats are benefited when there is not

much cough, and the patient sleeps well. Atropia will stop other forms of sweating, such as the sweating of acute rheumatism, prolonged suppuration, convalescence, &c. (Dr. W. Murrell, p. 84.)

Oxide of Zinc.—Probably no remedy has been more extensively employed in the treatment of the night-sweating of phthisis than oxide of zinc. The estimation in which it is held will be gathered from the following extract from Dr. C. J. B. Williams's well known work on consumption:—"The medicine we have found to act almost as a specific on night-sweats is the oxide of zinc in doses of two or three grains in the form of pill at night. This we have given ourselves and seen others give to thousands of patients, and the good results have generally been so prompt and lasting that in few cases has it been necessary to continue it for any lengthened period." The oxide of zinc is usually given at bed-time in from five to ten-grain doses made up into pill, with extract of henbane or conium. The hyoscyamus is said to prevent sickness, and probably exercises an influence allied to that of its more powerful congener, belladonna. (Dr. W. Murrell, Practitioner, August, p. 91.)

PHTHISIS.—*The Climate of Davos-Platz.*—The question the physician will ask himself is, What class of cases are likely to do well at Davos, and in what stage? the latter question depending on the former so far as this, that a case of a proper kind might hopefully be sent in a later stage than a case of a less appropriate kind. Speaking generally, the cases which do so well at Davos, and of course elsewhere also, are cases of acquired phthisis when the patient is yet young and in possession of a good frame and fair physical powers. Such a case, sent, say, with consolidation of one apex or a simple cavity, is tolerably sure of cure. And I believe such cases may be sent to Davos not without hope even in advanced stages. Passing over the many intermediate cases and turning to those with strong hereditary tendency, with red tongues and irritable stomachs, with diarrhoea at times, rapid emaciation, excited circulation, and sharp evening fevers, and perhaps with neurotic complications, of these I have less hope even when the pulmonary signs are limited. I think such cases should scarcely be sent at all when the signs in the chest are much advanced, or the presence of abundant tubercle suspected. To send prostrate emaciated patients of this class "to breathe stove air at Davos," as Dr. Bennet says, is quite unjustifiable; and what are we to say of those who send patients with diffused tubercle, and suffering perhaps in lung, larynx, and bowel? Let such false kindness be resisted; let

the physicians learn to speak the grave truth to such sufferers, and tell them how little they have to hope for from anything but the comforts of home. (Dr. Clifford Allbutt, p. 86.)

There is a fashion about health resorts as about most other things. For a time one particular place is recommended; after a while the reputation of this begins to wane, and another comes in favour. This remark applies especially to Davos-Platz, which it is now the fashion to recommend. Unfortunately but too often the recommendation of any particular place is, in the first instance, based upon an imperfect knowledge of the facts, and it is only when these have become fully ascertained that a reaction occurs, such as, I believe, will ensue in the case of Davos-Platz. It appears that the range of temperature experienced at Davos during the month of January 1879 was something enormous, it varying from 7.5° below zero up to 141.0° ; the radiation thermometer even, as already remarked, showing a difference of 107° . It is difficult to comprehend how such extreme variations of temperature can be otherwise than injurious in the great majority of detected cases of phthisis, which are mostly cases in an advanced stage of the disease. Patients are recommended to leave Davos not later than the beginning of April. But where can they go to at this inclement season? It is far too early to return home. A warm place, even were one readily accessible, but which is not to be found nearer than the south of France or the Riviera, would scarcely be suitable after the Siberian winter of Davos. Indeed there is reason to believe it will eventually appear that one of the great disadvantages of Davos is, that a winter passed there renders the frame subsequently unfitted for warmer and more temperate climates, and that, quitting its bracing and freezing atmosphere, there is great risk of the speedy loss of whatever increase of strength and health may have been derived from a winter there. (Dr. A. H. Hassall, p. 89.)

PNEUMONIA.—Aconite.—Aconite follows the universal law of agents (not merely therapeutical remedies) competent to affect the economy, viz., that it acts by *action* and *reaction*, which are contrary to one another. The action of aconite is the same as that of cold; it depresses the vital power of the sympathetic nerves, and contracts the vaso-motor muscular fibres. The reaction of aconite (or, properly speaking, of the economy affected by aconite) is shown in relaxation of the spasm, and in congestion of the capillary vessels. If one attends only to this latter occurrence, one may say, aconite causes the phenomena of the (simple) feverish state. In point of fact, however, this is only half the truth, which is,

stated fully, this: aconite causes first spanæmia, and, second, congestion of the capillary vessels. Now what is (simple) inflammation? Is it not the reaction due to exposing the body to cold? And are not the steps of the process first spanæmia of the exposed part, and second congestion of the same? The latter condition we are in the habit of calling inflammation, though only by overlooking the fact that it is a secondary, and not a primary occurrence—an error, by the way, that Cullen did not fall into. Aconite, then, cures inflammation because its action depresses the vaso-motor system, and therefore lowers the excitement of the febrile reaction. For which reason the dose should be small, so small as not to induce any secondary wave of reaction, which might leave the patient as bad as he was before. Being small, the dose should be frequently repeated; and I am in the habit of administering it, not every hour or half-hour, but every ten or fifteen minutes, till lowering of pulse and temperature, moisture of skin, and sleep, are induced. (Dr. A. C. F. Rabagliati, p. 76.)

Ergot.—Dr. Wells, of New York, states that in his practice ergot has yielded better results than any other treatment. In all of the ten cases in which he employed this drug, the “rusty” sputa was speedily and permanently arrested, and the attack in half the cases aborted; in others so shortened as to recover in six or seven days. This remedy, he states, acts as promptly in pneumonia as in hemoptysis, whether used hypodermically or per ora, and in a few hours arrests the rust by relieving the intense congestion on which it depends. He usually combines it as follows:—℞. Fl. ext. ergot, f. 3 iv.; tr. digitalis, f. 3 j.; plumbi acetatis, gr. vj.; aquæ cinnamomi, ad. f. 3 ij. Sig. Give a tablespoonful every two hours until bloody sputa stops, then twice a day. He begins the treatment by an antipyretic dose of quinine, from 40 to 60 grains, which, in connexion with the ergot mixture, equalises the pulmonary circulation, relieves the congestion and inflammation, and the patients recover in about half the time required by other modes of treatment. Believing that this treatment far excels all others in its rapidly beneficial results, he presents it to the profession for trial. Ergot has been previously employed in the treatment of pneumonias by Dr. Wycisk, who related in the Allg. Med. Central Zeitung, five cases in which he used ergot early, none ended fatally, none became chronic, and none left appreciable deposits behind them. In all of them the exudation was decidedly checked by the ergot. (Dr. J. T. Wells, Dublin Journal of Medical Science, June, p. 553.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

DYSENTERY.—Local applications, in subacute and chronic dysentery, by bismuth injections in the rectum have succeeded wonderfully, and a great many cases of hopeless chronic cases have recovered. I order half a drachm of subnitrate of bismuth, to be rubbed down with half a drachm of powdered gum in two ounces of cold water, and injected from once to three times a day, according to the severity of the case. The enema must be retained. The severe tenesmus and tormina are relieved in a very short time. (Dr. E. P. Houghton, Sarawak, Borneo, *Lancet*, Oct. 4.)

EXCISION OF THE TONGUE FOR CANCER.—Removal of the tongue, wholly or in part, for cancer, is a justifiable and a wise surgical proceeding, and the sooner the operation is performed after the diagnosis of the disease has been made, the greater are the prospects of a long immunity from the disease, or even of a complete cure. The division of the sensory nerves of the tongue was first proposed by Hilton, who practised section of the gustatory branch of the fifth nerve with a view of relieving the pain of a cancerous ulcer, and enabling the surgeon to apply ligatures for the removal of the cancer to a part that has been deprived of all sensibility. Moore also adopted it, and it undoubtedly accomplishes the object for which Hilton originally proposed and practised it, more especially so far as the relief of pain and diminution of salivation are concerned, and it deserves to be considered one of the most efficient modes of relief to the suffering produced by cancer of the tongue in all cases in which an operation for the removal of the disease is not desirable or practicable. Moore's method is to be preferred to that of Hilton (for the former divided the nerve further back), and becomes more applicable when cancerous disease lays hold of the floor of the mouth. The good effect of the operation is instantaneous. Pain ceases in the tongue, ear, face, and head, and the flow of saliva is greatly diminished; the relief besides, is continuous, the nerve appearing not to reunite. Nunneley, of Leeds, devised an ingenious mode of applying the *écraseur*, so as to remove large portions of the tongue as far back as the hyoid bone. His operation consists in passing the chain of the *écraseur* through the centre of the mylo-hyoid space by a needle into the mouth close to the *frænum*; two or three curved and strong hare-lip pins are now passed deeply into the tongue, obliquely behind the seat of the disease, their points being made to project forwards below the organ, so as to prevent the chain from slipping; the loop is now gradually tightened

and the tongue cut off obliquely from behind forwards. (Dr. F. A. Purcell, p. 168.)

HEMORRHOIDS.—*External*.—Bathe the part thoroughly with water, as warm as can be borne, together with the free use of Castile soap, and afterwards apply equal parts of the compound gall ointment and extract of belladonna. The operation must be repeated every three or four hours till the pain subsides. Usually the first application gives relief. Without the previous bathing with soap and warm water the application of the ointment is of little service. (Dr. D. Young, p. 180.)

Carbolic Acid Treatment of Piles.—Dr. Smith related in detail, two cases of piles, and mentioned shortly six others, in which he had used a *watery solution* of carbolic acid, with the most perfect success. He employs a fine hypodermic needle, the point of which is thrust into the centre of the tumour; ten or twelve drops of 1-20 or 1-30 solution of the acid are then forced in, till the pile is slightly distended. Trifling pain, sometimes only a little smarting, follows, and the patient is usually able to be out and at work the same day. In ten days the piles injected are at most half their former size, when a second operation may be necessary. Dr. S. injects only one or two tumours at a sitting. He makes no reference to the presence of any ulceration or slough, or hard cicatrix. There are two things that must be specially attended to; the needle must not be inserted too close to the base of the pile, and the solution of the acid must be perfect,—there must be no undissolved globules floating about in it. The conclusion arrived at by the writer is that “the injection of weak carbolic solutions is a means that can be safely employed in completely internal hemorrhoids, or those that are more or less continually protruding from the anus.” (Dr. Smith, p 182.)

HERNIA.—*Strangulated Hernia reduced “En Bloc.”*—There should be little difficulty in arriving at a decided diagnosis between an internal stangulation and a hernia reduced *en bloc*. As to treatment, the sooner operative measures are resorted to the better, and as to the method of operating, I feel satisfied that the old or direct method is better suited for cases of hernia reduced *en bloc* than the mesial incision. Let us briefly consider the advantages and disadvantages of each method. In the direct method, by incising in the line of, and laying freely open, the canal through which the hernia formerly protruded, we are certain of finding the constricted hernia as it is fixed by its connexion with the peritoneum, and if the incisions be properly planned, we generally find it

easily, and can readily bring it down into the canal. Then we deal with it as in an ordinary hernial operation, opening the sac and examining its contents, to satisfy ourselves of their condition, and of the propriety of reducing them or otherwise. Next, by dividing the constriction to a moderate extent, we feel whether the gut is adherent, and if not, we can draw it gently down so as to see the part which has been more immediately under the constriction (for that is the point of danger), and judge of its condition and its fitness for reduction. And all this is done whilst the parts are, as it were, outside the abdominal cavity, and therefore with little or no risk of fæcal extravasation. Again, if, as is too generally the case in hernia reduced *en bloc*, we find the intestine gangrenous or parts of it in a doubtful state, we can deal with it much more satisfactorily than if we had opened the sac from within the abdomen, and had to deal with gangrenous gut in that cavity. It not unfrequently happens in hernial cases in which constriction has continued for some time, that changes have begun in the gut which was directly under constriction, sometimes pretty firm through recent adhesions, with softening of the gut in the immediate vicinity. In other cases the state of the strangulated intestine may be such as to render its reduction of doubtful propriety, even if not adherent. We know that though the peritoneal aspect of the intestine is not gangrenous, that the mucous membrane at the seat of constriction suffers at an earlier period, and is liable to ulcerate and lead to perforation, with its fatal consequences, if the doubtful portion of bowel has been returned within the abdominal cavity. In such cases the proper plan is to relieve constriction freely, but not to reduce the doubtful portion of bowel immediately. Here it is evident that we can examine and deal with the contents of the sac much more safely and satisfactorily, by what I call the direct method than by a mesial incision, by which we can only reach the strangulated intestine indirectly, and cannot see or judge of its state at or below the constriction until we have freed and brought it into the cavity of the abdomen. The mesial incision in cases of strangulated hernia reduced *en bloc*, besides being more dangerous than the old or direct method, seems to me to present no advantage whatever, except to resolve a doubtful diagnosis. (Prof. Spence, p. 176.)

INTESTINAL OBSTRUCTION.—1. The operation of opening the abdomen is only to be performed where certain previous treatment has failed. 2. It is only to be performed antiseptically, so that the risk of septic peritonitis may not be added to the list of dangers against which the patient has already to struggle.

3. But, above all, the operation must be performed earlier than has heretofore been the case. It is hopeless to perform it when symptoms of peritonitis or enteritis have set in. Those who wait, as I venture to say too many have waited, till the abdomen is generally and enormously tympanitic—till the temperature is persistently high, or is only falling before the inevitable end—till the pulse is running down and the patient in a condition of irrecoverable collapse; those who wait till any or all of these conditions are present, had far best not operate at all. The strength of the solution to be used for spray and sponge must not be above 1 in 40; the solution must be duly warmed. If the operator be obliged to draw any coils out of the abdomen for the purpose of examination, the spray must not be allowed to play upon these coils, which are to be covered with carbolised lint, taking care that the smooth and not the fluffy surface of the lint is in contact with the intestine. If any amount of carbolised solution be believed to have entered the peritoneal cavity, this is to be removed by careful sponging; and finally, in closing the wound, catgut being unreliable, twisted silver sutures are to be employed. The patient is to be fed by enemata, given *per rectum*, and consisting of milk and egg, wine or brandy, and given carefully twice or three times daily. If possible, only ice out of milk is to be sucked by the mouth. If possible, no opium is to be given, as this drug has a dangerous tendency to mask symptoms; instead of opium, belladonna is to be given, and to be pushed in large doses. At an early date, the method of *abdominal taxis* should be made use of. Mr. Hutchinson, in bringing this method forward, recommends that it should be carried out as follows. Under chloroform, a very copious enema of water is to be given by a long tube, and, the anus being kept closed round the tube with a cloth, the fluid should be forced in to the utmost point of distension. Then, simultaneously with the withdrawal of the tube and the escape of the water, the surgeon, with the flat of one hand on each side of the abdomen, should press gently but firmly on alternate sides, in such a way as to facilitate the movements of the coils upon each other. As much of this having been done as shall seem advisable, and the water having flowed out, let the patient (by means of a girth fastened to the bedposts) be raised by the feet till the trunk is inverted; and whilst the patient is in this position, let the surgeon, with both his hands placed on the lowest part of the abdomen, press the whole mass of the intestines as high up as possible in the abdomen. Of course, during the whole of the time an assistant should carefully watch the effects of the chloroform on the pulse and breathing. I have only one small

alteration to suggest in the above details ; and that is, knowing the difficulty of inverting a heavy patient, I think it will be found better to overturn a chair, so that its sloping back be against the bed ; over the chair-back let some clothes be laid, and the patient's body be gradually inverted over this by assistants standing on the bed. This will not only be found easier in the case of a heavy patient, but in that of a female one more decent in the eyes of any friends who may be present. (Dr. W. H. A. Jacobson, p. 188.)

NUTRITION.—*Calcium Phosphate.*—The importance of this substance is shown by the fact that phosphate of lime is in largest proportion in those animals whose activity is greatest, and whose temperature is highest. Phosphate of lime administered in an insoluble state passes along the alimentary tract, and is for the most part ejected with the fæces without causing any marked change in the animal economy. An entirely different action takes place, however, when the phosphate is dissolved in lactic acid. Under the form of lacto-phosphate, it stimulates the function of nutrition, whether in the adult or in the infant. In the latter under the influence of this substance the weight of the body undergoes a regular and progressive increase. Whilst exercising this general recouping influence, lacto-phosphate of lime exerts a special effect upon the osseous system, in which it causes an increase of hardness, or in cases of fracture, consolidation. This double action is the basis for the therapeutic applications of lacto-phosphate of lime. In rickets, M. Dusart finds that in every case in which the diet, though sufficient in quantity, was unsuited to the digestive organs, the addition of lacto-phosphate of lime caused rapid improvement. (M. Dusart, Practitioner, May, p. 374.)

PODOPHYLLIN.—*Formula for.*—All who have been accustomed to prescribe podophyllin in pills will agree as to the impossibility of preventing occasional disastrous effects. But this is the fault of the form of administration, not of the drug. From a very long and extensive experience I can confidently affirm that none of the accidents and inconveniences which so commonly attend the administration of podophyllin ever arise when the drug is prescribed according to my method. On the contrary, it is one of the most satisfactory and reliable of our medicines. The formula given is: *Rx.* Podophylli, gr. ij. ; essentia zingiberis, 3 ij. ; spiritus vini recti ad 3 ij. Fiant guttæ. A tea-spoonful to be taken in a wineglassful of water every night at bedtime, or every second, third, or fourth night as required. (Dr. Horace Dobell, British Medical Journal, June 14, p. 892.)

SEA SICKNESS.—*Nitrite of Amyl*.—As the nitrite of amyl is now very highly spoken of as a remedy for sea-sickness, I should like to give my experience of it in one case, and at the same time, and more importantly, to decry its indiscriminate use by the public especially. It is now sold in boxes of glass capsules, for which an exorbitant price is charged, and the dose put up in those I have seen is far too large. I gave one of the capsules to a gentleman on board ship, who was feeling very sick and ill. I saw the establishment of the physiological action in the flushing of the face, and he at the time told me he saw a number of black specks before his eyes. But in his case violent vomiting and retching came on at once. Knowing that there was an overdose in each capsule, I did not allow him to inhale the whole of it. It may be, however, that he got an overdose, and that nature resorted to vomiting to rid herself of the poison. But, again, the effect is only temporary; the sympathetic recovers its tone over the vessels, and the patient is as he was before. Is it then advisable to continue inhalation two or three times in the day? The effect of the remedy is so disagreeable that for my part I would rather get over my day or two of occasional vomiting without it. The dangers connected with its indiscriminate use are, in my opinion, great. In cases, for instance, of diseased cerebral arteries, the rapid and extreme dilatation would, I should say, tend very much to produce rupture. (Dr. C. R. Illingworth, Madeira, Lancet, Aug. 2, p. 184.)

AFFECTIONS OF THE URINARY SYSTEM.

ALBUMINURIA.—*Diagnostic value of*.—Since the observations of Prof. Leube of Erlangen, we must believe that transient albuminuria may occur in perfectly healthy persons; and Dr. Moxon and Dr. Dukes have told us how persistent it may be under the conditions they have described. I have tested these urines on each appearance of the patients, and have proved its persistence for weeks and months. The microscopical examination has revealed usually nothing. Albuminuria in phthisis is well enough known; but it must not be supposed that even under these conditions it indicates amyloid or fatty degeneration of the kidneys. Albuminuria in epilepsy was first described by Max Huppert; and more recently Otto found albumen in the urine twenty-two times in thirty-one cases after epileptic attacks. The occurrence of albuminuria in bronchitis calls for no remark, except that a large number of cases of bronchitis have been included under granular kidney, it being so frequent a coincident or consequence of the latter disease. Albuminuria in connec-

tion with tonsillitis has been noticed. In one of the cases, the condition was chronic, and might be classed with those considered under the head of debility. So that albuminuria is a *symptom* of many diseases, but it is a phenomenon which may be present under such a variety of conditions that *per se* it affords us no indication of renal disease. (Dr. R. Saundby, p. 96.)

CYSTITIS.—*Contagion through the Use of Instruments.*—Cystitis is capable of being propagated by the direct transference of inflammatory products from the bladder of one patient to that of another. All are sufficiently familiar with the fact that purulent matter from the vagina, and probably from the uterus also, produces inflammation of the male urethra, and that conjunctivitis may be caused by contact with pus from either source. All metal instruments—catheters, sounds, and lithotrites—after use, at any rate in cases of muco-purulent urine, should be plunged for a minute or two into boiling water, to which either a little common soda or a little carbolic acid has been added. If the boiling point of water be not considered absolutely sufficient, a strong solution of chloride of zinc in water may be used. At the strength of 12 per cent. solution, the boiling-point is 220 deg. Fahr., or eight above that of boiling water. For some years past, as advised in the last edition of my lectures, I have always placed all gum and other catheters and bougies in a bath of weak carbolic acid immediately after use. I have more recently—that is, since the occurrence described—added a solution of carbolic acid to the oil used for the lubrication of instruments. Oil being the remedial agent for the caustic effects of carbolic acid, there is no danger in applying to the urethra a comparatively strong solution of the acid in oil, since no irritating effect whatever is produced, and the disinfectant influence is unimpaired. For the last two months I have used the following formula, and can, therefore, guarantee that it is absolutely unirritating: *Rx.* Acidi carbolici med. gr. xii.; olei olivæ ℥i. A free use of this as a lubricant to all instruments before using will, I believe, ensure, at all events in combination with the modes of cleaning just described, safety from the occurrence of any contagion by means of instrumental treatment. (Sir Henry Thompson, p. 192.)

NEW SUSPENSORY BANDAGE.—The common suspensory bandage has these disadvantages: (1) if it is not fitted with a perineal band, the genital organs are pulled so far forwards as to cause an unsightly prominence beneath the trowsers; (2) if it has the ordinary perineal band, the anus gets chafed; (3) the tape in the hem round the margin of the bag chafes

and cuts; (4) the perforations in the material of the bag are too fine for ventilation, consequently the bag becomes soaked with perspiration; (5) the hole for the penis is unnecessary, and only serves to let one testicle occasionally herniate through it. The bandage I wish to bring under notice is contrived to obviate the above objections. A median perineal band holds the genitalia well back. An india-rubber ring in this band prevents it from chafing the annus, and the anus from soiling it. Another india-rubber ring takes the place of the piece of tape above referred to. The bag is hand-made, knitted, and very open in its network; and the penis is simply allowed to hang over the front of the bag. The contrivance has been freely tried in cases of gonorrhœa, orchitis, varicocele, relaxed condition of the genitalia attended by hypochondriasis, and in other affections of those parts, and found to answer perfectly. Messrs. Arnold and Sons, of West Smithfield, make the appliance in three sizes, at the price of the old bandage. (Mr. Charles B. Keetley, *Lancet*, May 24.)

STRICTURE.—*New application of the Gum-Elastic Catheter.*—Most surgeons have probably experienced the difficulty, in cases of tight stricture, that arises from the extreme flexibility of the small-sized gum-elastic catheters; for, while they, on the one hand, double up and fail to accomplish their purpose, silver instruments are objectionable from their too great stiffness. To obviate the difficulty, I adopt the following plan, which seems to answer admirably. A gum-elastic, large enough to admit the passage down it of a No. 1 or 2, is cut off about three inches from the point and smoothed. The small instrument is passed down it to the cut end, and both are introduced together as far as the stricture. By keeping the outside catheter firm while the inner one is gently pressed on, one has all the advantages of a flexible *point* attacking the constricted canal in the *centre*, and the instrument is supported right up to the point of action. (Dr. W. Peel Nesbitt, Surgeon to the Burra Burra Hospital, South Australia, *British Medical Journal*, Dec. 7.)

FRACTURES, DISLOCATIONS, AND DISEASES OF BONES, JOINTS, &c.

ABSCESSSES.—Like others, I used to be a strong advocate for a free incision, believing that if that was not done we should have sinuses formed. But that is now no longer necessary with drainage tubes and antiseptic arrangements. The tube does far better than a free incision. The latter might have its edges glued together with lymph or clots, and so be altogether inadequate, but with the tube this cannot be. And

while the pus or serum is taken freely away, being prevented from putrefying, it is itself unirritating, and no longer provokes, as formerly, a copious further effusion. The smallness of the incision is, of course, in various ways a great advantage. In the case of a young lady, for instance, with an abscess in the neck, we wish to avoid making any mark. We used to make a free incision, but now a puncture just large enough to admit a small tube, will be quite sufficient, and will leave a punctiform scar which nobody can observe. (Prof. Lister, p. 104.)

ANTISEPTIC SURGERY.—*Carbolic Acid*.—The way in which it mixes with various materials gives carbolic acid great advantages otherwise. A new principle in pharmacy seems to have been brought out by our experience with this agent. I believe it is generally assumed by pharmacists that the strength of any given agent in its action depends upon the proportions in which it is present in the vehicle or solvent—that if the agent exists in twice as great amount, it will be twice as strong in action. Now that is true in the same solvent, but it is not true with different solvents. Thus carbolic acid is soluble in 20 per cent. of water. But oil can be mixed with it in any proportion. Suppose you compare a 1 in 20 solution in water with a solution of 1 part of carbolic acid in 5 of oil, you will find that the solution of 1 in 20 of water is more powerful in its immediate action than a solution of 1 in 5 parts of oil. The solution in oil contains four times as much of the agent as the watery solution, and yet is not so powerful in its action. You can bear the 1 to 5 oily solution applied to your tongue-tip or the thin skin of the lip better than you can the 1 to 20 watery solution; and the 1 to 20 solution in oil is so bland that it can be used for smearing a catheter or lithotrite without irritating the delicate mucous membrane of the urethra, to which the 1 to 20 watery solution would be intolerable. Now the reason appears to be simply that water holds the carbolic acid feebly, and so readily lets it fly off to attack anything else. Oil, on the other hand, holds it strongly, and therefore lets it go with difficulty. With common resin we have the opposite extreme to water. If you melt 1 part of carbolic acid with 5 parts of resin, you find that you can hardly perceive the taste of the acid at all in the mixture. I at first supposed that some sort of decomposition of the acid must have occurred, but I found that such was not the case at all, only the carbolic acid was more strongly held by resin than by oil. Now I say that is highly interesting as a new principle in pharmacy—that the energy of action of any caustic or irritating substance depends, not

merely on the proportion in which it is present in its vehicle, but also in the degree of tenacity with which its particles are held by it. But now as to the application of this principle in the employment of carbolic acid. The watery solution, besides being cleanly, is very powerful in immediate operation, but as there is not a large quantity present in the solution its action is not of long duration. Now those are just the properties we want for a detergent application. We want something that will act powerfully for the time, but transiently. Suppose a patient comes into hospital with a compound fracture of the leg—there septic mischief has already been introduced into the wound by contact of the injured tissues with objects of the external world, and what we want is to destroy the septic organisms already present in it. We wish to do this by means of some germ-poison that shall act powerfully, but that, as soon as its work is done, shall disappear, so as to leave behind as little effects of irritation as possible. For such a purpose, then, we use the 1 to 20 watery solution. Again, in our operations we must have an agent that shall produce an antiseptic atmosphere—that shall be strong enough to purify the air in which it is diffused in the form of spray—but that shall not irritate the wound to an unnecessary degree. If the solution used in the spray were like the solution in oil, it would continue to act for a long time afterwards on the tissues. But the watery solution, acting only for a short time, purifies the atmosphere without seriously irritating the tissues. For a permanent dressing, on the other hand, we want a preparation that will not act too energetically. It must keep the septic material out of the wound; but as long as that condition is complied with, the less it acts the better, or it will irritate the skin. Further, it must not be too transient in operation, otherwise it will lose its efficacy before the dressing is changed. And so for a permanent dressing we require some vehicle which shall present it mild in its operation, but persistent. For this purpose we use resin. In this gauze which I have here we have carbolic acid blended with resin, with a little paraffin added to prevent the mixture from being too glutinous. Each thread of the gauze is occupied with this resinous mixture. The resin is itself insoluble in the discharges, cannot be washed out, and then again the resin holds the carbolic acid so tenaciously that the discharge may go through over and over again without washing all the acid out. So between the two we have what we require. We have the carbolic acid present in a form in which it acts sufficiently but mildly, while it cannot be dislodged by any amount of discharge that is likely to occur in twenty-four hours, and if the dis-

charge comes to be extremely slight, as in the later stages of a wound, you may leave the dressing unchanged for a week. Beyond that I do not generally trust it; but for a week you may trust it with absolute confidence if the discharge be small. Now as to the manner in which this gauze is applied. We must have a sufficient mass of it. I shall make up a dressing as for a case in which I expected a large amount of discharge—as in psoas abscess. We generally have about eight folds of gauze. I now make up a dressing of nine layers, which is often a convenient form, the piece employed being as long as the breadth of the tissue. This piece, about a yard square, I fold into three, and then crosswise into three again, so getting nine folds. It is essential, if you expect much discharge, that the dressing should be large—that there should be a considerable space between the wound or source of discharge and the edge of gauze dressing, otherwise you will not have material enough to ensure the absence of putrefaction. If I were to put it on as it is—gauze, and nothing more—the discharge would come directly through the porous material, and passing over and over through one limited portion, would probably, in spite of the retentive property of the resin, wash out all the carbolic acid from that part before twenty-four hours had expired; and as soon as the antiseptic had thus been removed from that portion of the gauze putrefaction would spread in. I therefore interpose under the outermost layer some impermeable tissue to prevent the discharge from traversing the gauze directly, and compel it to travel through its entire breadth. The best article for this purpose is a very thin mackintosh cloth, which is very durable, and can be used again and again for weeks together, and thus in the long run is as cheap as it is trustworthy. I take a piece of this material as large as the folded gauze, and put it under the outermost layer. It is made of pink colour, rather than white, to make it conspicuous and show which side of the dressing is to be kept outwards. If you put it outside altogether it is liable to be displaced. Then you should take care not to make any pinholes in the centre, near the wound. This may seem a small matter, but there is room enough for I do not know how many bacteria to go abreast through a pinhole. (Prof. Lister, p. 99.)

Antiseptic Dressings with Boric Acid.—Dr. Solger says that he uses boric acid in the antiseptic treatment of wounds in the following way. The cotton-wool which is going to be used in the dressing, is plunged into a 10 per cent. watery solution of boric acid, which is warmed to a temperature of 50° R., then taken out and allowed to cool down to 35° to 40° R., put on

the wound, which has been previously thoroughly disinfected, and kept in its position by another layer of dry cotton-wool and a bandage. The high temperature of the dressing has a hæmostatic effect on the wound. According to the manner in which it is used, boric acid will either increase or lessen the property of cotton-wool, allowing the secretions of wounds to filter through it. If a plug of cotton-wool be soaked in a 15 to 20 per cent. solution of boric acid at a temperature of 60° R. and above, then allowed to cool down to 35° and spread out over the surface of a suppurating wound or abscess, or a fresh wound, and fastened by means of dry wool and bandages, the boric acid forms, on evaporating, a large quantity of boric acid crystals; at the same time the wool adheres so firmly to the skin that it entirely excludes the air and remains thus for months. On the contrary, the wool will allow the secretions to filter through it, if it has been soaked in a mixture of boric and carbolic acid (five parts of boric and two parts of carbolic acid, and 100 parts of water). Boric acid dressings will be found very useful in the minor surgical operations. (Dr. Solger, London Medical Record, June 15.)

Antiseptic Use of Thymol.—A lecture on this subject has been published in Volkmann's series, by H. Ranke, of Halle. The solution used instead of the 3 per cent. solution of carbolic acid, consists of one part thymol, ten parts of alcohol, twenty of glycerine, and a thousand of water, and can be employed as either a spray or a solution. An impregnated gauze is also used. Since thymol does not irritate the wound, the gauze may be laid directly upon it; otherwise the same method is employed as in Lister's plan. If the gauze becomes hard and dry, it may be moistened once or twice a day with thymol water. In order to prevent the evaporation of the thymol from the dressing, the gauze is covered with oiled paper. From an experience of forty-one wounds dressed with thymol, the lecturer concluded that the method leaves nothing to be desired as to its antiseptic effect, and that it answers better than the carbolic acid dressing, since the secretion from the wounds is less, the period of healing shorter, and the cost of the dressings is smaller. Further, it has no poisonous properties, and eczema was never observed in its use. (H. Ranke, Medical Press and Circular, Aug. 28.)

DRILLING AND TREPHINING BONE THE SUBJECT OF CHRONIC INFLAMMATION.—The operation of dividing longitudinally by a saw or of trephining bone which is the subject of chronic inflammation, has been practised at intervals by surgeons for many years. And yet if the principle on which the practice is based be right, the operation should have a wider

range; for granting that the division or trephining of a bone chronically inflamed *relieves tension*, gives exit to inflammatory fluids, lessens pain, and cures disease in such obstinate cases as have resisted all other modes of treatment, it is not unfair to infer, if the same, or an allied practice were applied at an earlier period of the inflammatory affection, that equally good results would be obtained; and that while in some cases the inflammation would be checked by the operation, in many an early and complete recovery might be anticipated. In acute periostitis or endostitis, a free incision down to the bone, by relieving tension and giving exit to inflammatory effusion, does nothing but good; and that it should be made as early in the progress of the case as the diagnosis will justify, and if possible before pus has formed. The very commonly fatal termination of these cases by blood poisoning, when they are left to run their course unchecked, renders this measure imperative. That in all forms of endostitis, or osteo-myelitis of long bones, in which more or less intense and persistent pain is a prominent symptom, the operation of drilling, trephining, or making a free opening into the bone should be entertained, as any one of these measures tends to check the progress of the disease, and in most cases relieves pain. In flat bones, such as those of the head, and in cases in which the preceding measures seem too severe, the simpler operation of cutting down upon the bone and separating the periosteum from it should be performed. That in all cases of *suspected* abscess in bone, the same operative proceedings should be carried out, the operation of trephining inflamed bone suspected to be the seat of suppuration being generally as successful in relieving pain and effecting a cure as it is well known to be when a local abscess in bone is found to exist. (Mr. Thomas Bryant, p. 125.)

FRACTURE.—*Pain as a Symptom of.*—The pain produced by motion may be of great use. It is logically evident that if we can impress motion on an injured but healthy bone by means of force applied at a distance from the part hurt, and without in any way disturbing its surroundings, the pain so elicited must mean fracture and nothing else. In the case of *fracture of the ribs*, having ascertained that the pain on pressure of the injured part is situated over one or more ribs (if it be not so, there can be no fracture), the surgeon should trace these ribs to a distance of some inches. When at this distance from the bruising, firm pressure is made, severe pain will be developed at the fracture, if there be fracture, little or none if there be not. It is only necessary to be accurate as to the bone pressed upon. In the case of *fracture of the*

thigh bone, the true way to save the surgeon's credit is to make a correct diagnosis at the first—not to hedge by stating that there is only a bruise, but that such bruises are prone to produce shortening. This diagnosis may be made in two ways. One method consists in giving chloroform, and so moving the leg as to elicit crepitus, when you will also probably find that you have succeeded in producing shortening and eversion. The other, and, as I think, the better mode is, having weighed well all the circumstances, to confirm the diagnosis by the character of the pain produced by movement. I have not yet satisfied my mind by a sufficient number of cases, but I am nearly certain that, if the pain caused by inversion be considerable, a fracture has to be dealt with. Pain on eversion and flexion are also greater with fracture than with bruise, but the distinction does not appear to be so marked. Tenderness or pain on pressure over the injured part is invariably present in all cases of fracture. It may be definitely affirmed that this tenderness is experienced round the whole circumference of the bone in the line of fracture. The question is, How can this law be practically applied? Its usefulness varies according as the fracture has been produced by direct or indirect violence, and I think it will be found that as a negative sign it is decisive, if not always clinically valuable, and that as a positive indication of fracture it is also of great importance. In fractures produced otherwise than by direct violence, pain on pressure attains its highest value. The same circumstances as in direct fracture, no doubt, impede more or less the precision with which the fact can be ascertained. The bone may be so deep or so situated that it cannot be directly pressed upon in the whole or some part of its circumference, and injury of the soft parts in the neighbourhood may under certain circumstances be produced by indirect violence. But these impediments are not so serious as in the former case. If any part of the contour of a bone be subjected to pressure without pain it is certainly not broken on that particular level. And this is of practical value especially in the vicinity of some of the joints. The elbow, for example, has been violently wrenched. By putting the arm in the easiest position, and then running the finger along the exposed edge of the ulna, pressing together the condyles of the humerus and their ridges, and feeling the head of the radius, you may distinctly determine that there is no fracture without further examination. And, on the other hand, the depth or situation of the bone does not necessarily impede the discovery of fracture by this sign. It is not essential that the whole circumference of the bone should be exposed to pressure. A.

very small portion will suffice, if it be immediately under the finger, if the painful spot be in the usual line of fracture, and if the cause of the injury be undoubtedly indirect. Take a common example of fracture from indirect violence, Pott's fracture of the fibula. Even though there be no displacement, the diagnosis of fracture may be made with certainty, if there be pain on pressure above the external malleolus, and especially if this be accompanied by tenderness over the internal lateral ligament of the ankle-joint. It is only, therefore, when the bone is altogether so situated that the pressure applied loses in precision that this circumstance interferes with the usefulness of the symptom. (Dr. John Duncan, p. 117.)

Plaster of Paris Splint.—I have used the following mode of dressing in my last seven cases, three of fractured thigh and four of the leg:—A dry bandage of German gauze, three inches wide, is firmly bound around the limb, the latter having been *thickly* padded with cotton-wool only upon either side (to the padding is added, if strength is particularly required, one or two layers of millboard). When two complete thicknesses of gauze have been smoothly applied, a layer of "plaster-cream" is smeared on about 1-16th of an inch thick; two more layers of gauze are applied, another layer of "cream" is laid on, and the splint is complete. The plaster is, of course, only put upon the sides of the limb, and the two joints contiguous should be enclosed as is generally recommended. As soon as the plaster is dry, it should be split up, a common roller applied and drawn tight, and the patient put upon crutches. I have had simple fractures of the leg walking within twenty-four hours after the reception of the injury. The only advantage I claim for the splint is its great lightness; while it is very solid, and fits, necessarily, the limb to perfection. (Dr. Oscar J. Coskery, Baltimore, U.S.A., *Lancet*, August 2.)

HOUSEMAID'S KNEE.—*The Seton.*—The seton, composed of a double silk thread moistened in weak carbolic oil, is conveniently passed through the same canula which draws off the fluid, and in certain circumstances, where the avoidance of pain is urgently required, the ether spray will be found a great boon. Too small a trocar is not to be selected, or the apertures will be apt to close up and obstruct the discharges. To have to enlarge them afterwards would be exceedingly disagreeable. A pad of lint, moistened also with carbolic oil, covered with gutta-percha tissue, and the whole secured by a few turns of a bandage, is both agreeable to the patient and helps to maintain the patency of the apertures. The seton

should be drawn every morning, in order to present a fresh portion of it each time to the suppurating interior, and the pus encouraged to ooze out by frequent and gentle pressure with the fingers. In five or six days the seton may be withdrawn, and after five or six days more of rest the patient may be allowed to walk about. Should any congestion or weakness be left behind, it is effectually overcome by the local use of the cold douche. Iodine, blistering, pressure, and even simple tapping, are very uncertain remedies, and their employment in nearly every case is merely a source of additional annoyance to both the patient and the practitioner. (Dr. J. A. Austin, p. 134.)

MINOR SURGICAL OPERATIONS.—*Primary Anæsthesia of Ether.*—

For the short operations, the reduction of dislocations or opening of abscesses, the primary effect of ether is extremely useful and of every-day application. When you wish to operate on a patient without causing him pain or incapacitating him from attending to his business for the remainder of the day, let him lie down on the sofa and take the ether inhaler—a sponge wet with ether—in his own hand, directing him to hold the other arm up in the air. After breathing the ether for a few minutes, the arm will drop, and you will have from thirty to fifty seconds of unconsciousness in which to operate. The sponge is removed, and the patient is ready to go about his business. It gives rise to no headache, nausea, or other unpleasant symptom, and is particularly useful in children. The chief source of disappointment is in not recognizing the right moment, for, if this is allowed to pass, unconsciousness will not again occur until full etherisation. The first insensibility is sure to come. When the arm wavers, be ready, and as soon as it drops perform the operation; there will be no pain felt. (Dr. J. H. Packard, New York Medical Journal; and Glasgow Med. Journal, April.)

SUTURES.—*Silkworm or Fishing-gut Sutures.*—The chief excellence of fishing-gut consists in its causing little or no irritation when embedded in the tissues. The way in which the latter tolerate its presence is wonderful, far surpassing either fine silk or silver wire, and, being perfectly soft while contained in moist structures, it remains pliable, admitting of, and participating in, the movements of these, instead of being stiff and resisting like wire. Another valuable quality is its comparative indestructibility, in this greatly differing from cat-gut, which, after a few hours, becomes completely softened and disintegrated, and finally incorporated with the surrounding tissue. Fishing-gut maintains its integrity for many weeks, its strength seeming in no way impaired after removal.

Its peculiar structure seems to render it almost as incapable as wire of becoming impregnated with the discharges from wounds which so often make silk injurious, if not absolutely dangerous. I am not aware of any other material that can be introduced into and remain embedded in the tissues for so long a time without causing suppuration or else losing its integrity. I therefore believe that when these properties of silk-worm gut are more widely known it will be highly valued by surgeons. Mr. Bryant writes me that he has used nothing else for plastic operations for the last ten or twelve years. (Mr. J. H. Walters, p. 161.)

New Elastic Suture.—The following elastic suture is recommended by Dr. Vogel for closing a gaping superficial wound, and for drawing the edges of the latter together. Wide strips of sticking-plaister are placed on both sides of the wound, from one to two inches from the edge. Several small holes are then made in that portion of the strips which is near the edge of the wound, and small-sized studs are placed into the openings. A narrow India-rubber band is then laid across the neck of two opposite studs, slightly tightened, and fastened. This new suture is said to have answered very well in cases where the metallic suture either caused suppuration or could not be applied because the edges of the wound were too far distant. (Dr. Vogel, British Medical Journal, June 21.)

WOUNDS.—Taking a case—say of amputation through the thigh, or of excision of the breast—I should treat the wounds in the way following. Having carefully arrested all hemorrhage, using most probably the carbolised catgut ligature, and having gently removed any particles of blood-clot that may have lodged on the surface, employing only clean water or sponges just rinsed out of it, I should, without any further interference with the surface of the wound, bring the edges together, adapting these as nicely as possible with silver-wire sutures. I should not in this way attempt to close the wound completely, but I should leave spaces between the sutures, perhaps from one to two inches long. Then, over the course of the wound and for some distance on either side of it, I should place a layer of folded lint which had been previously well soaked in olive or almond oil containing one part in about fifty of carbolic acid. Over this again I should place two or more layers of dry lint, either with or without cotton-wool; so arranging this as by gentle and equable pressure to secure, without any violence, as far as practicable, the accurate adaptation of the surfaces of the wound throughout, avoiding thus any considerable cavity in the interior. I should secure all this by strapping or bandage, or both, so adjusting these

that they may be hereafter removed with the least disturbance. I should place the patient and the wound in the most comfortable position possible, having especial care to the fact that fluids as they form may flow outwards. Thus for instance, after excision of the breast, I have, for some time past, placed the patient, not on her back, but on the opposite side, so as to make the inner angle of the wound the most dependent part of it. I am convinced that this assists greatly in promoting speedy and satisfactory repair. It is much better for fluid to escape at the inner than at the outer angle, and this more especially when the axilla has been disturbed by the removal of glands. The cellular tissue is so very much less abundant and less loose towards the sternum, that the chance of infiltration of the fluids is very much less. As a rule, perhaps, I do not disturb this arrangement for forty-eight hours, although very often I change the dressing and inspect the wound after twenty-four. I am guided in this matter of time chiefly by the state of the patient; whether spare or full-bodied; her sense of local and general comfort, freedom from or complaint of pain; and the season or temperature. But whenever I am in any doubt, I change the dressings. These, then, are removed with the utmost gentleness, and the state of the wound carefully inspected. Especially is attention directed to whether there is any tendency to the lodgment of fluid; whether that which forms can escape freely; whether there is much tension of the edges. I am bold enough to think that any surgeon who understands his business can tell, without any painful handling, whether the surfaces of the wound are fairly in contact, or whether there is any tendency to the accumulation of fluid separating them. But if any doubt arise on this important point, a perfectly clean probe or director lightly applied to some portion of the wound will solve it, and secure ample vent; if at all necessary, I should not hesitate to remove one or two or more sutures. If the wound presented no other evidence than that of satisfactory repair, I should dress it as before, and proceed in this fashion, dressing and examining it daily or less frequently, according to circumstances. But if at the first dressing, or whenever afterwards the discharge became at all profuse, or the surfaces did not remain in contact, or there were much tension or a blush at the edges, I should forthwith substitute a bread-and-water poultice for the previous dressing, and probably continue to apply this until at least all the deeper portion of the wound had closed. When I dressed the wound, I should wash it probably from the first with tepid water, perhaps containing some permanganate of potash in the form of Condy's fluid or other potent antiseptic of the least irritating kind. I should accomplish this washing out,

if I thought fit, of portions or even the whole of the interior by the use of a syringe, avoiding contact of sponges or other substances with the wound. I aim here at the utmost possible cleanliness, having at the same time due regard to the avoidance of any unnecessary disturbance, that the process of repair be not interrupted. And withal I endeavour, by means I need not indicate, to secure for my patient the most complete rest and the purest air. (Mr. W. S. Savory, p. 107.)

AFFECTIONS OF THE SKIN, ETC.

ACNE.—Dr. Gage Parsons believes that Mr. Erasmus Wilson was the first to propose sulphur lotion in acne punctata. The usual lotion of flower of sulphur with glycerin and water is undoubtedly a valuable remedy; but from the readiness with which the sulphur separates it is inelegant and inconvenient, whilst it is not quite satisfactory in its results. A far more efficacious mode of using sulphur is to dust the face with pure precipitated sulphur every night, with an ordinary puff used for toilet purposes. This will usually effect a cure in about a week. Recently two severe cases of acne, of two years' standing, which had resisted the ordinary methods of treatment, yielded at once to sulphur thus applied. If the sulphur be scented with oil of lemon or roses, it will form an elegant cosmetic. (British Medical Journal, June 7.)

For acne, Dr. G. Law, U.S.A., advises that the patient should use common carbonate of soda and common soap in washing; this dissolves and removes impacted sebaceous matter. On drying the parts the following lotion is to be applied:—*Rx.* Acidi acetici ʒiv., glycerini ʒii., aq. rosæ ʒiss. (Glasgow Medical Journal, April.)

BALDNESS.—*Pilocarpine.*—It is announced by Dr. G. Schmitz (Berl. Klin. Wochens.) that pilocarpine possesses, in a remarkable degree, the power of reproducing the hair on a bald surface, in certain instances at least. The cases on which this statement is based are as follows:—A man, 60 years of age, completely bald (with the exception of a few white hairs on the occiput), was operated upon for double cataract. After the operation there remained, in one of the pupils, a fragment of membrane, to cause the absorption of which the author administered, within a period of fourteen days, three subcutaneous injections of the muriate of pilocarpine. The membrane disappeared; but this was not all. At the same time the head became covered with a thick crop of hair, which grew so rapidly that, at the end of four months, no trace of baldness remained. The new hair was partly white

and partly black. The second patient, aged 34, suffering from separation of the retina, presented on the crown of his head a perfectly bare patch, as large as an ordinary playing card. Two injections of the same substance not only cured the ocular affection but produced an abundant growth of hair on the bald part of the head. Unfortunately, Dr. S. gives no information as to the nature or causes of the baldness in his two patients. (Glasgow Medical Journal, June.)

CHRONIC ULCERS OF THE LEG.—*Martin's bandage*.—Since the commencement of the present year I have used with excellent results the solid rubber bandages introduced by Dr. Martin of New York. The bandage has been put on next the skin without any other dressing: the patient has been instructed (1) to remove the bandage on going to bed, (2) to wash the leg with warm soap and water, (3) to sponge the ulcer with carbolic acid lotion and keep this applied all night, (4) to have the bandage well washed and hung up unfolded during the night, (5) to again wash the leg in the morning, and (6) to apply the bandage before leaving bed. If applied in suitable *chronic* cases the bandage is more efficacious than any other method of treatment. It is, I think, a most valuable addition to the appliances used in minor surgery. (Mr. A. F. McGill, p. 226.)

The bandage of "pure rubber" alone, without any other application whatever, sufficed for the best possible treatment of ulcers of the leg. Carbolic acid in various degrees of dilution—hinders, greatly hinders, the process of cicatrization. The bandages which I use and recommend are of what is technically known in this country as "pure rubber"—i.e., the very best old "seasoned" Para India-rubber, *intimately* combined with the extreme minimum of sulphur absolutely necessary, with a proper degree of heat to accomplish the perfect "vulcanisation" or "curing" of the gum. These ingredients must not be combined by solution of the rubber in naphtha or any other solvent, but by a process in which the rubber and sulphur are *thoroughly* combined without any other solvent or substance whatever. Bandages containing a particle of any material besides these two are *not* "Martin's bandages," and I disclaim, utterly and decidedly, all responsibility for effect, or want of effect, observed in their use. (Dr. H. A. Martin, p. 228.)

Sheet Lead.—Dr. Atkinson recommends the application of sheet lead moulded to the shape of the leg and kept on by an ordinary calico bandage. The size of the lead should be sufficient to cover the ulcer completely and lap a little over

the whole skin ; the edges and angles should be well rounded, so as not to chafe or irritate ; it should be about an eighth of an inch in thickness, and moulded very accurately to the shape of the leg, so as to allow of no indent being apparent on the surface. After it has been carefully fitted, the leg should be bandaged from the toes upwards, and all that then need to be done is to uncover the ulcer night and morning and allow some water from a sponge to trickle over it. The granulations should never be touched with the sponge itself. (Dr. F. P. Atkinson, p. 227.)

CHRYSOPHANIC ACID IN SKIN DISEASES.—1. Chrysophanic acid is a powerful local stimulant—not, however, tending towards vesication or ulceration. Its action in this capacity is best illustrated in psoriasis. 2. It also possesses undoubted parasiticide power. 3. It is a most efficient remedy in two parasitic affections—viz., ringworm of the body, and tinea versicolor. 4. In ringworm of the scalp it frequently fails owing to causes that militate against all remedies as yet tried. 5. In favus it has not, so far as I know, as yet had a trial. 6. As a destroyer of animal parasites its efficacy remains to be proved. It would probably be of service in some cases of scabies and in pedicularia. 7. Internally, as Dr. Ashburton Thompson has shown, it is an emetic purge. 8. Alizarin, upon several grounds, appears to deserve a more extended trial in diseases of the skin. (Dr. Walter G. Smith, Dublin Journal of Medical Science, March.)

ECZEMA.—*Oleate of Zinc*.—I have always used the oleate of zinc made into an ointment, either with vaseline or with lard. The preparation with vaseline I have employed in private practice, and that with lard, on account of its comparative cheapness, for my hospital patients. Vaseline is preferable to lard, because it is not so liable to change, but also because the greasiness of the latter injures a patient's clothes and sometimes disagrees with his skin. The ointment made with vaseline may be rendered more “elegant” by the addition of one drop of otto of roses to each ounce. My house-physician, Dr. John Wood, informs me that he has found the oleate of zinc ointment very serviceable in the treatment of eczema, and especially in the eczema capitis of children. One of my patients, a sexagenarian clergyman, the subject of eczema, probably gouty, of several years' standing, affecting the lower part of the abdomen, the genitals, and the upper portions of the thighs, tells me that he recovered completely after using the vaseline ointment for a few weeks. (Dr. J. Sawyer, p. 225.)

Eczema of the Hands.—The writer relates the treatment he successfully adopted in a case of most intense eczema of the hands and feet. The hands were alternately bathed with glycerine and covered completely with compound lead ointment (the form I always use is given in Mr. W. Spencer Watson's book on Diseases of the Nose and its Accessory Cavities); and light thread gloves were constantly worn. Only the ointment was applied to the feet. At a later stage, the parts were washed with milk and sulphur soap; and towards the end of the treatment arsenious acid was ordered in the form of pills. At the expiration of about three weeks, my patient left Bath much better; he got through the terrible winter without serious drawback, and in February he was virtually well. For some time, he used the ointment occasionally; the smoothness and flexibility of the palms of the hands are perfectly restored, and he can walk any reasonable distance with ease and comfort. (Dr. J. K. Spender, p. 223.)

LEPROSY.—*Chaulmoogra Oil.*—There are some points of clinical importance to be observed in the administration of chaulmoogra oil. It should be given at first in small doses of three or four minims, which, as the stomach becomes more tolerant of it, may be increased to a drachm. It should always be taken after food. It is apt to produce constipation and sickness. This is best combated by administering some mild purgative to ensure the regular action of the bowels, when the tendency to nausea generally disappears. In leprosy, at least, a much larger dose than that ordinarily given is required. I am in the habit of prescribing the oil as a mixture suspended in gum, or as an emulsion. It may be more elegantly administered in *perles* or capsules; and I have had some made containing fifteen minims each, which I find useful and convenient. Chaulmoogra is the oil expressed from the seeds of the *Gynocardia odorata*. Its melting-point being high, it is semi-solid at the ordinary temperature. Its taste and odour are not disagreeable, and it comes to us with a high Indian reputation as a remedy for scrofula, skin-diseases, and leprosy. (Mr. Wyndham Cottle, British Medical Journal, June 28.)

PRURIGO.—*Injection of Carbolic Acid.*—Fleischmann vaunts the efficacy of subcutaneous injections of a 2 per cent. solution of carbolic acid in cases of prurigo. At first he injects subcutaneously half, and subsequently the entire of, the contents of an ordinary Pravaz's syringe. The injections are made in those places where the itching is greatest, and the number of the injections should be regulated by the gravity of the case, and especially by the intensity of the itching. In Fleischmann's cases the total number of the injections varied from

three to fifteen. One injection at least was made every second or third day. The relief, followed later by a perfect cure, was never delayed longer. (Dublin Journal of Medical Science, March.)

PSORIASIS.—*Pyrogallic Acid*.—Dr. Besnier and Dr. Arragon, of Paris, have employed pyrogallic acid in the treatment of psoriasis, with good results. The acid is mixed with simple ointment, in proportions varying from 5 to 25 per cent, and this ointment is applied by means of a stiff brush once or twice daily on the diseased patches, either just as they are, or after they have been well washed with black soap. It may also be applied upon a piece of linen by manual rubbing, or by a wad of linen impregnated with the acid. Care should be taken to prevent its admixture with soap or other substances containing alkalis, for, if this should occur, the acid decomposes and gives rise to undetermined black products. The mean duration of time to effect a cure is four weeks. They find that it is not inferior to any of the topical remedies in use for psoriasis; that, while its action is relatively somewhat slow, it is not offensive, and when applied of an ordinary strength, does not produce erythema to such an extent as to interrupt the cure of the affection, having succeeded in a case in which chrysophanic acid had to be discontinued owing to the irritation it caused. It produces a more or less dark colour of the skin, which passes off in a few days, and it seems also to deepen the shade of the hair. It has no smell. Its price is somewhat less than that of chrysophanic acid. This treatment is especially suited to cases of obstinate psoriasis, in which other methods have failed. It is well at the commencement to order but a single rubbing in one day, and not to prescribe two successively until it is found that the medicine is well borne. As a rule baths are valueless, but their occasional use does not appear to be counter-indicated. Arsenic to be taken internally may be prescribed simultaneously with the pyrogallic acid. (Drs. Besnier and Arragon, Glasgow Medical Journal, June.)

VASELIN AND UNGUENTUM VASELINI PLUMBICUM IN SKIN DISEASES.—Professor Kaposi, after stating that all emollient substances hitherto used in diseases of the skin where the epidermis is removed or the surface is sensitive,—as various fatty substances, oils, lard, glycerine, and glycerine of starch, are more or less irritating in most cases—refers to the bland and non-irritating properties of vaseline or petroleum jelly (with this also may be classed ozokerin and unguentum petrolei). These have no tendency to become rancid, and are

useful in softening and removing crusts and scales, as in cases of eczema squamosum when the surface is dry and desquamating. He introduces an ointment which promises to prove of great value. This is a modification of Hebra's well-known unguentum diachyli, which is seldom met with properly prepared except at Vienna. This ointment, for which Kaposi proposes the name of Unguentum Vaselini Plumbicum, is made by dissolving and incorporating thoroughly by aid of heat equal parts of lead plaster and vaseline, to which a little oil of bergamotte may be added to scent. It causes no burning sensation on excoriated parts, and is especially available in eczema. It is admitted by Kaposi that the original unguentum diachyli gave rise to unpleasant heat and even acute exacerbations of the eczema, due, he believes, to an evolution of fatty acids from the oil during boiling, and to an imperfect saponification of the oxide of lead. (Prof. Kaposi, Practitioner, Feb., p. 124.)

VENEREAL AFFECTIONS.

CHANCRES.—*Iodoform*.—Mr. Berkeley Hill in a short paper which appeared in the British Medical Journal, Jan. 26, 1878, and in his Students' Manual of Venereal Diseases, published recently, says: "All chancres are best treated with iodoform: under its use healthy sores heal rapidly, creeping sores generally cease to spread, and sluggish ones take on healthy action." My own experience supports this statement most fully. I cannot explain the manner in which it acts, but that it does have a most remarkable effect in promoting the healing, not only of ordinary chancres, but of many other sores, I can have no possible reasonable doubt. It is what I might call a *reliable* remedy, and often saves one a deal of trouble. Its effects seem almost magical. You sprinkle a little of the crystals, powdered or unpowdered, over the sore, cover this with a bit of dry lint, or vaseline spread on lint, and at your inspection next day you find that healing has progressed rapidly, the sore has filled in considerably if it is a deep one, there is but little discharge and no smell, and you have only to repeat the dressing, and so go on from day to day until healing is complete. The relief from pain is often remarkable. (Mr. Berkeley Hill, Dr. Alfred Sheen, p. 314.)

GONORRHOEA.—For gonorrhœa, Dr. G. Law, U.S.A., orders an injection of one ounce of tannic acid, in six ounces of water. to be used in the way specified below:—The patient is first made to urinate, to wash out the accumulated matter, and then, a No. 6 or 7 catheter having been passed beyond the

point of soreness in the canal, he is directed to make firm pressure on the tract of the urethra beyond the end of the instrument, so as to close the passage. Now, by means of a syringe, the nozzle of which fits the catheter, the urethra is to be washed out with cold water; when this is thrown in with some force the return current flows out at the meatus round the catheter. Finally, inject the tannin solution in the same way. This should be repeated twice a day, gradually weakening the solution. (Glasgow Medical Journal, April, p. 330.)

Emollient Injections.—Dr. Bauer, of St. Louis, says, I rarely resort to internal treatment, since the disease is simply local. The injection which I apply in the acute cases is as follows:—
℞. Inf. sem. lini (ex 3 iij. parati), ʒvj.; cui adde ext. opii aquosi, fl. gtt. xvij. M.S. To be injected warm every three hours and retained for a few minutes. This injection is not only emollient, but sufficiently viscid to cover the bare urethra with a protecting coat, and sufficiently narcotic to soothe the irritated nerve papillæ. It is advisable first to clean the urethra with a warm water injection. Without exception, the patients at once felt relieved, and the discharge commenced to diminish. Towards the end, the injection might be alternated with a very weak solution of the acetate of lead, say one-third of a grain to the ounce of liquid, which seems sufficient to dry the secreting surface and to complete the cure. (Dr. Bauer, London Medical Record, May 15.)

SYPHILIS.—Treatment without Mercury.—For several years, I treated syphilis, in the majority of cases, without the use of mercury, internally or externally; and in all such cases, without any exception, the ulterior results were most highly satisfactory. All my cases of syphilis were treated by hot baths. Whatever variety there may have been in the use of other remedies, there was none in this. *Hot baths was the standing order for all cases of syphilis.* And, for the most part, the men were strictly confined to bed, often notwithstanding their entreaties to be allowed to get up. The most scrupulous attention was paid to the cleanliness of their linen. Such local applications as black wash, calomel, red precipitate, or citrine ointment do not in the least degree, in my opinion, diminish the value of the cases in establishing the fact that syphilis can be cured without the specific action of mercury. In my cases, the actual amount of mercury imbibed by the system must have been very small indeed; the remedies were not at all pushed, quite the contrary; and anyone who could attribute to their use the good results which followed must be a believer in the Hahnemannian

doctrine of infinitesimal doses. It must be remembered that advocates of mercury contend that, without approaching the direful effects of salivation, a cure can only be effected by introducing a considerable quantity of the mercury into the system, and by protracting its use over a long period. My sheet-anchor has in all cases been the frequent employment of hot-water baths; the patients being a good deal confined to bed, the most scrupulous attention being paid to personal and surrounding cleanliness, full hospital diet always allowed, and with some exceptions, a pint bottle of the best ale daily. Iodide of potassium took second place in the treatment, and other salts of the same alkali were freely used at times. (Dr. J. P. H. Boileau, p. 196.)

UNHEALTHY LOCAL AND SYPHILITIC SORES.—*Prolonged Immersion.* The method of treating phagadænic and other ulcers by more or less prolonged immersion of the patient in warm water is by no means new, having been employed for many years by Hebra in syphilitic and other diseases of the skin. The following is the plan of treatment which was followed at the Male Lock Hospital:—The patient sits in an ordinary hip-bath, containing sufficient water to ensure constant submersion of the affected part, for eight or ten hours a day. If the disease be in the groin, a full-sized bath, in which the patient can recline, may be necessary. The temperature of the water is regulated by means of a thermometer, and is kept as near 98° F. as possible by the removal and addition of small quantities of water at frequent intervals, without disturbing the position of the patient, and in winter the bath is placed near a fire. The exposed parts of the body are covered with blankets. In the evening, finely-powdered iodoform or other dressing is applied, and the patient goes to bed as usual. Next morning the dressing is allowed to separate in the bath, the pain attending its removal being thus avoided. The bath is repeated day after day as long as may be necessary, general treatment, according to the nature of the sore, being carried on at the same time. A good purge is beneficial at the outset, followed by iron, quinine, or ammonia and bark with opium, in local sores, and by appropriate specific remedies when the patient is syphilitic. A liberal diet, with plenty of milk, and little or no alcohol, answers best as a rule. Though men only are here referred to, it need hardly be said that the same treatment is equally applicable to women. In order to obtain the full benefit from immersion it is necessary to bear in mind a few practical points. First, care must be taken to see that the affected part is thoroughly and constantly submerged; the penis as

a tendency to float, and if the supply of water be too scanty, and the extremity of the organ be the seat of the ulcer, this may be altogether above the surface, and thus failure will occur. It is also imperative that the whole of the unhealthy part be exposed to the action of the water; if phimosis prevent this the foreskin should either be slit up or removed altogether. When the prepuce is simply divided the edges of the wound always swell more or less, and sometimes so much that the glans becomes almost buried, the discharge does not get freely away, and the length of time necessary for immersion is prolonged; besides, a subsequent operation to remedy the deformity is generally necessary. Two lateral incisions, as recommended by Dr. R. W. Taylor, answer very well in completely exposing the parts. Some of the advantages of immersion are: its effect in nearly always quickly relieving, and often in removing entirely, the severe pain attending phagedæna. Caustics are very rarely required. The ulcer is kept clean and free from discharge without pain or trouble. When a wound has to be made by the surgeon, its edges hardly ever become inoculated. The pain caused by frequent change of dressing is altogether avoided. The materials necessary for immersion being only those usually to be found in every house, it can be equally well carried out in private as in hospital practice. (Mr. Arthur Cooper, p. 201.)

AFFECTIONS OF THE EYE AND EAR.

CONJUNCTIVITIS.—*Method for the Rapid Relief of Pain after the Application of Copper Sulphate.*—Dr. Carl Pick has noticed that the application of sulphate of copper to the conjunctiva causes severe pain. In conjunctivitis blennorrhœica this application has to be made daily for several months. It has occurred to the author by a lucky chance that calomel may be of use to relieve the pain. He therefore proceeds as follows. He applies the bluestone in the ordinary way to the conjunctiva, and after an interval of four or five minutes he sprinkles a little calomel over the places where the application has been made. The pain immediately disappears. After adopting this method for five or six days the calomel may be used directly after the application of the sulphate of copper, when the pain will disappear instantly. The author is unable to give any reason for the disappearance of the pain under these circumstances. (Dr. C. Pick, Practitioner, June, p. 457.)

DEAFNESS.—*A New Acoustic Apparatus.*—The structure of the ear, constituted as it is of vibrating membranes, of ossicles with joints, of fluid, and of vibrating parts in connection

with the nervous elements, points to it as a mechanical organ, and as one likely to require treatment on mechanical principles. For a stiff joint we prescribe exercise, for a rusty hinge we employ motion. If the muscles and joints of a pianist becomes impaired in their delicacy of movement, we prescribe much practice. By use, the hinge comes to work sweetly, and the muscles and joints often re-acquire their lost powers; so it is reasonable to infer that the stiff membranes, joints, and other structures of the ear, being brought into exercise by vibratory motions sufficient to set them in play, may advance more and more to a condition of health. Not only is "hearing better in a noise" an absolute truth, but it is perfectly capable of explanation, and is a most important guide to treatment in many cases. If the hearing be better in a noise, why not let the patient have more noise? The deaf can usually bear a good deal without inconvenience. What would stun a healthy ear only brings many deaf persons to the hearing point. I have at present a young gentleman under my care, who is so deaf that, whilst he is in a quiet room, I must speak distinctly and close to the ear to cause him to hear, but, whilst walking in the street or riding in a railway train, he can converse with me with facility. I recently asked a lady whether she heard better in a noise; she replied that she heard quite well in a noise. Her daughter, who accompanied her, and whose hearing was acute, assured me that her mother heard much better in a noisy street or in a railway train than she did herself; in fact, it was only in quietness that her mother was deaf. Do the laws of physics not explain this apparent anomaly? Is the improvement not owing to the fact that parts whose rigidity has been overcome to some extent by strong vibrations, more readily respond to the minor impulses of the human voice? It is well known that vibrations of varied degrees of intensity and pitch may all at the same time traverse the same media, each system preserving its individuality. We know that it is difficult to set a laden wagon in motion, but once the inertia has been overcome, small forces may afterwards materially contribute to its advance. So with the rigid structures of a diseased ear. The ear is quite analogous to other parts of the body. The most perfect development of bones, muscles, and joints, can only be brought about by much work. Weak and atrophied muscles and stiff joints are the invariable result of want of exercise. To have a perfect organ we must have exercise and plenty of it. If this be necessary to maintain the functions of a healthy organ, how much more is it so to restore an impaired one! The uselessness in general of all acoustic apparatus hitherto in use will be at once apparent to

anyone who knows the history of deaf persons : they are practically debarred from social intercourse, their auditory organs lie fallow, and their mental powers become very much blunted from want of training in the great school of the world. The idea occurred to me that I could accomplish this by making the furniture of home and office acoustic apparatus. Ninetenths of a man's time is spent at home or business ; and if during this period I could secure the easy exercise of the function of hearing, the advantage resulting to the deaf would be incalculable. By this plan it is clear that very large tubes may be used ; but it was essential not only to use large tubes, but have them so arranged that the terminable nozzles should remain in the ears, when introduced, without requiring to be held in place, that they should not press on the ears injuriously, and that the head might be moved without hurting the ears. I required a tube of sufficient rigidity to maintain its shape and position, so that it would not require the aid of the hand to keep it in place, of sufficient flexibility to permit of its being moved freely in every direction on very slight pressure, without materially diminishing the bore of the tube, and of sufficient diameter to admit of a large volume of sound. It should have a smooth internal surface so as to have as little irregular reflection of sound as possible. It should admit of being shortened on slight pressure on the nozzle, so that if the head were moved against it the tube would shorten, and the ear be thus saved. I was much puzzled for some time as to how I should obtain a tube having all these properties, some of which almost seemed to be antagonistic to the others. At last I took a lesson from nature. It occurred to me that the trachea possessed all the properties specified, and I therefore made a tube, taking it for a model. On a shape of the desired form, a thin layer of India-rubber was moulded, and on this a spiral of the same material was coiled externally ; rings would also suit. These tubes may be made of many varieties, curved, straight, cylindrical or conical. The woodcut (*vide page 220*) illustrates the form I think best. On trial, I found that the tubes fulfilled all my requirements, and rendered a binaural apparatus capable of easy use. Both ears may be used at the same time. Deaf persons, whilst the apparatus is in use, are put more nearly on a par with hearing persons as regards the exercise of the function. Deaf persons are enabled to attend to business from which they would be otherwise precluded ; for example, a judge on the bench, a lawyer in his chambers, a merchant in his office, may perform their respective duties by aid of an acoustic chair or acoustic writing-table. (Dr. W. A. McKeown, p. 214.)

FUNGUS IN THE EAR.—It is extremely probable that in many cases the fungus is derived from the syringe, and not from the ear. It is not improbable that, when a growth of fungi in the syringe takes place, spores may be introduced from the syringe into the meatus, and there give rise to otomycosis, especially as the syringe is so frequently used in diseased states of the ear. Two practical conclusions seem to follow from the above. 1. It behoves every one, removing material from the ear which is suspected to contain a fungus, either *not* to use a syringe for that purpose, or else to ascertain the absence of fungi in the instrument. 2. It is advisable that all syringes employed in syringing the ear should be examined occasionally, to see that no growth of fungi takes place in them, which, by the introduction of spores into the meatus, might give rise to some form of otomycosis. If it be not advisable or possible to remove and cleanse the piston after each time of using the syringe, washing out the interior with a weak solution of carbolic acid or thymol or with hot water suggests itself as a suitable means of preventing the growth of fungi; or lubricating the piston, in the first instance, with oil or vaseline containing carbolic acid, would perhaps answer the purpose equally well. It is probable that, in syringes in which the soft covering (leather) does not extend to the lower surface of the piston, a growth of fungi is less likely to occur than in those constructed as above. (Dr. E. Cresswell Baker, *British Medical Journal*, March 22, p. 426.)

MIDWIFERY, ETC.

ERGOT.—*The Action of.*—Dr. Peton has studied experimentally and clinically the physiological and therapeutic action of ergot. The experiments have led him to conclude that ergot has a direct elective action upon unstriated muscular fibre. If it is desired to obtain its action rapidly upon a given organ, the drug must be injected hypodermically in the neighbourhood of that organ, since its effects take some time to develop after administration by the mouth. Ergot does not act solely upon the muscular fibres of the uterus, as it affects also the muscle of the bladder and of the vessels. Ergotised rye, which is most generally employed for obstetric purposes, is also of great use in metrorrhagia, hemorrhage from the alimentary canal, hemoptysis, and epistaxis. Dr. Peton believes that the activity of different solutions is very variable, and that a solution which contains a gram of ergot in a cubic centimetre of fluid is the most useful preparation. (*Le Progrès Médical*; *Practitioner*, July, p. 52.)

FORCEPS (THE) AND ITS ALTERNATIVES IN LINGERING LABOUR.—

The following propositions present the points that chiefly challenge discussion. 1. In lingering labour, when the head is in the pelvic cavity, the forceps is better than its alternatives. 2. In lingering labour, when the head is engaged in the pelvic brim, and when it is known that the pelvis is well formed, the forceps is better than its alternatives. 3. In lingering labour, when the head is resting on the pelvic brim, the liquor amnii discharged, and it is known, either by exploring with the hand or by other means, that there is no disproportion, or only a slight degree of disproportion, even although the cervix uteri be not fully dilated, the forceps will generally be better than its alternatives. 4. In proportion as the head is arrested high in the pelvis, in the brim, or above the brim, the necessity, the utility, and safety of the forceps become less frequent. 5. As a corollary from the preceding proposition, increasing caution in determining on the use of the forceps, and greater skill in carrying out the operation are called for. In most things there is a middle way. “Ni jamais, ni toujours,” is a proverb full of wisdom. I cannot better illustrate the wisdom of deducing the greatest good from over-caution on the one hand and from too bold enterprise on the other, than by citing the precept and practice of Boër. This famous surgeon, having witnessed in Paris the extreme activity of French midwifery, and in London the too procrastinating practice of England, recognised the middle course as the best, constructed his forceps of medium length, saying, “Everything is not to be taken away from Nature, neither is everything to be left to her.” (Dr. Robert Barnes, *Lancet*, May 17, p. 692.)

It is necessary in considering this subject to distinguish between what McClintock calls the “high” and the “low” forceps operations. The low operation takes simply those cases in which the head has entered the pelvic cavity; the high operation applies to those cases in which the head is seized at or above the brim. The distinction is imperative. The low operation may be accomplished by the short or single curved forceps. It is usually a very simple proceeding. The presence of the head in the pelvis implies, generally at least, that there is no obstacle from disproportion to be overcome, that the resistance of the cervix uteri and vagina has been overcome, and that there remains little or nothing beyond the resistance of the vulva. We merely want a little *vis à tergo* to complete expulsion. In this conjuncture some will prefer ergot, some will prefer forceps. My own decision has long been in favour of the forceps. I hesitate to affirm that

the use of ergot is wrong. But the following reasons for preferring the forceps are to my mind sufficient:—1. Since the publication of Hardy and McClintock's report of their researches at the Rotunda, which showed that unless the child were born within a short time after the action of the ergot it was very likely to be born dead, I have turned my attention to this point, and have seen abundant confirmation of the accuracy of their observations. 2. The action of ergot upon the uterus is uncertain. You want a certain amount of uterine action. You give ergot, and the desired result may ensue. But it may not. You may have too much, or too little. In the first event you have evoked a Frankenstein whose brute violence you can neither subdue nor regulate. The force which should be delicately adjusted to the resistance is in excess. And then you have to fear the very dangers of lingering labour—rupture of the uterus, other injury to the soft parts, and the death of the child—which you called in the ergot to avert. I prefer to enlist an ally which will do exactly what I want, and no more. 3. In a large proportion of cases of arrest of the head in the pelvis, the arrest is due to malposition of the head—for example, to occipito-posterior positions. The occiput turns up under the sacral promontory, and then the driving force propagated along the child's spine tells in a false direction, tending to roll the head more over on its transverse axis. It tends, in short, to revolve in extension around the promontory as a centre, instead of round the symphysis pubis. The driving force is wasted. Fortunately it often ceases or moderates. It is utterly unscientific, even dangerous, to goad it by ergot. The forceps is the true and effective help. It not only supplies the wanting force, but gives that force its proper direction, under conditions in the highest degree adapted to secure the well-being of mother and child. (Dr. R. Barnes, *Ibid.* p. 690.)

Where the liq. amnii has been sometime evacuated, the os being still not quite full size, the uterus sometimes becomes irritable. It compresses the child, but does not drive it on. Expectancy is only letting the uterus drift further and further into this disturbed and difficult action. If you give opium you can give some relief, chloral still more, chloroform still more; but there is a remedy beyond all these, and that is nitrite of amyl. There is nothing that allays uterine or other muscular spasms so well as that; in two or three minutes you may subdue the trismus, relax the uterus, get on the forceps, and deliver the patient. If we want to practise the high operation we must have the forceps long

enough. The Scotch instrument is not long enough. Dr. Simpson's has a double curve, which is a great improvement; but the double curve does not make the short forceps long. I have used it when I have not had an instrument of my own. It will not do where the head is lying upon or above the brim of the pelvis. It will do for brim operations; but not beyond that. With regard to the argument based on the instrument itself, I will quote what Dr. Simpson says in his notes of his lectures: "Never apply the forceps until the os uteri is fully dilated, and until the head is in the pelvis." There is the practical commentary upon his forceps. He tells you to follow the practice that is within the power of his instrument, and that is very natural. We have an illustration of the same point in his celebrated papers upon turning in contracted pelvis. The reason why he turned was because he had not forceps long enough to get at the head, and he had no resource but to turn. That is why turning was advocated by him in preference to forceps. Here in London, in Dublin, and in the larger towns, if the head is upon the brim of the pelvis and accessible to the long forceps, we prefer dealing with the child's head first instead of turning it. (Dr. R. Barnes, *Lancet*, July 19, p. 86.)

If any aid is necessary to midwifery, I feel that the forceps is superior to any other. With respect to the administration of ergot, it is absolutely given up in the Rotunda—I may say prohibited, because we do not allow it to be given in the extern maternity any more than the hospital. With respect to the compression of the abdomen, that is a practice which I must say I consider at least unscientific and calculated to be very injurious. As to the introduction of the fingers in the rectum with a view of expelling the head, it is a proceeding calculated to injure the soft parts of the mother; it is most irksome to her, and I suppose equally irksome to the practitioner himself. But on this point—that is, the use of the forceps when the head is low in the pelvis—I think that there is nearly a unanimity of opinion, as far as I can judge from reading the discussion which has taken place in this Society. The real question at issue is whether the use of the forceps is justifiable in cases in which the os is not fully dilated. Now, I do not think it is possible that any practitioner of experience who has thought over this subject would say that the forceps should not be used because the os is not fully dilated. I use it, but in as few cases as possible. When the head is low in the pelvis and the os fully dilated, we have no hesitation in applying forceps, and, I believe, rightly so: but when the os is not fully dilated, no matter

whether the head is above the brim or, as is sometimes the case when a foetus is small, when the head is low in the pelvis, I object to use forceps unless the condition of the mother is such as, in my opinion, warrants its use. A certain number of practitioners under-estimate the danger which may occur from too early use of the forceps before the os is fully dilated, and I believe that is due to the fact that they do not distinguish between the two very different classes of cases with which they have to deal. In some cases the os is so soft and easy dilatable that delivery is effected with the greatest possible ease and safety. In some cases it appears to me that to talk of the os not being fully dilated is really hardly correct. Over and over again I have watched a case in which the os could be distinctly felt. Keep your finger in contact with the os uteri, and as the pain comes on it will all but disappear; but if the pain is inefficient, the head will speedily recede as the pain goes off, and the os contract to the same condition as before. I have watched such cases a considerable length of time, and these are those cases in which one is tempted simply to apply forceps—a perfectly safe proceeding, but a most unjustifiable one; but it is far different with another class of cases. I think we are too much in the habit of talking of the “os uteri being dilated,” or “undilated,” as if the os were a mere rim, to dilate which all difficulty was over. But in point of fact we should in most cases speak of the “cervix” rather than of the “os uteri;” the whole lower segment of the uterus has to dilate during labour, and it is this undilated cervix which often opposes us. I have more than once applied the long forceps when I thought I had to deal with the os only, and found that my efforts to deliver were opposed by the condition of the cervix. The whole uterus descended as traction was being made, till the lip of the os appeared at the very ostium vaginæ. These were cases in which delivery had become imperative; but consider the pressure to which the cervix must have been subjected between the child’s head and the pelvic wall before delivery could be effected, and how great was the danger of laceration of the cervix. This induces me to avoid the use of the forceps before the os is fully dilated in all cases in which I can do so; but, on the other hand, if a case occurs in my practice in which I believe it imperative to deliver the woman before the os is fully dilated, I unhesitatingly have recourse to the use of the forceps, notwithstanding that the os uteri is not fully dilated. I believe that practice is safer than the practice of version, and I say that with a certain degree of confidence from a not very limited experience. I know the difficulty which I have experienced more than once

in endeavouring to bring down the head through an undilated cervix, which has tightly grasped the foetus round the neck, and I have no hesitation in saying I prefer the forceps to version under such circumstances as those. (Dr. Lombe Atthill, *Obstetrical Journal*, July, p. 241.)

By early use of the forceps is meant their use within the only partially dilated os uteri. We look to the Rotunda Hospital reports to see the result of the so-called "timely" interference. The figures answer with appalling clearness. The result of the practice has been to *more than double* the maternal mortality. At the rate of mortality which followed Collins' practice (1 in 100) Johnston should have had 78 deaths. He lost 169 patients—91 in excess of what might have been expected had the cases been treated on the principles of Collins. One patient out of every 47 died. Taking into consideration all the concurrent advantages which Johnston's patients enjoyed, this number is so large as to be terribly free from ambiguity. This death-rate demonstrates, in a manner which we hope it may never be necessary to repeat, the danger which the mother runs from the uncalled-for use of forceps. There is one other result which demands attention. It is the effect of forceps upon the child after delivery. Of the children delivered alive by forceps under Johnston, one out of every 11·5 died soon after birth. Of those born alive under Collins's management, only one out of every 58½ died before the mother left the hospital. The difference is great and, we think, important. This part of the subject is one which needs careful consideration. (Editor of *Medical Times and Gazette*, p. 273.)

It seems fairly agreed upon, that the practice recommended by Dr. George Johnston of applying the forceps to the head above the brim before the os is dilated, and sometimes before even the membranes are ruptured, cannot be supported by the mass of obstetricians at large. Most of the speakers during the recent discussion at the Obstetrical Society of London, and among them Dr. Grailly Hewitt, endorsed the fourth proposition laid down by Dr. Barnes in his masterly address, that "in proportion as the head is arrested high in the pelvis, in the brim, or above the brim, the necessity, utility, and safety of the forceps become less frequent." Dr. Grailly Hewitt admitted that the cases in which it is necessary to apply the forceps to the head above the brim do occur, but they are few. As regards Dr. Barnes's first proposition, that "in lingering labour, when the head is in the pelvic cavity, the forceps is better than its alternatives," the concensus of opinion was, as might have been expected, almost universal. It was gener-

ally agreed that, in such a case, the termination of labour by the application of the forceps was not only a simple and easy operation for the practitioner, but a humane and beneficial procedure for the mother and the child. (Editor of British Medical Journal, p. 271.)

INVERSION OF THE UTERUS.—Prof. White, of Buffalo, U.S.A., says: “Whilst undergoing this change (*i.e.*, involution), the uterus does not possess the firmness and elasticity of the unimpregnated uterus, nor the muscular flexibility and toughness of that at the full period of gestation. Indeed, I am induced to suspect that, at this period, the uterus cannot be subjected, without danger of laceration, to manipulation which would be perfectly safe at a later period, after complete involution has taken place.” So that if, from any cause, reposition of the inverted uterus be not effected within twenty-four hours after delivery, it is better to delay the attempt for some weeks, till the involution of the organ is completed. This opinion naturally leads me to the conclusion that mere lapse of time does not materially add to the difficulty of the operation. The attempt at reduction being decided on, and the time fixed for the operation, it is next necessary to consider the treatment to be employed. Numerous methods have been suggested; but that advocated by Dr. White is the best. “By means of the ‘repositor,’ uniform and gentle pressure can be maintained until the os is fully dilated and the fundus pushed up through it. The insurmountable difficulty heretofore has been supposed to consist in our inability to maintain uniform and persistent pressure for a sufficient length of time. The hand would soon become fatigued, and another hand, even of the same individual, could not be substituted without losing a part of what had been gained. This loss is increased when the hand of a fellow-practitioner is introduced to continue the operation. The various substitutes which have heretofore been resorted to for continuing pressure when the operator has become exhausted have utterly failed. The elastic bags, so often called in requisition, press more upon the viscera resting upon the large surfaces anteriorly and posteriorly situated, than upon the fundus, which has no firm ossific base of support, as have the rectum and bladder. The uterus ascends very soon, owing to the yielding nature of the vagina and perineum, and escapes from the reach of the distended vaginal bags. By means of the large spring at the outer extremity, the amount of pressure can be graduated to an ounce. The disc of this instrument will follow up the fundus, without compressing painfully the urethra or rectum, by means of this continuous elastic pressure in the

upward direction, until the fundus disappears in the os and neck. Any intelligent assistant can be trusted to increase or diminish the pressure during the absence of the operator, as the exigencies of the case may demand. The instrument (*see woodcut, p. 243*) is composed of a stem of wood or hard rubber, curved to conform to the vaginal curvature, with a coil of steel wire attached to the outer extremity, while the other end is expanded and hollowed so as to receive the fundus of the uterus in its concavity or disc. The edge of this disc is tipped with soft rubber, being an inch and three-eighths in diameter and about half an inch deep. The concave extremity of this instrument is carried up into the vagina and placed in contact with the fundus, and then firmly held by the hand in the vagina. The outer end of the instrument, or coil of wire, is placed against the breast of the operator on the same level with the uterus. By means of this large circular spring, the instrument readily keeps its place on the clothing of the operator, and leaves the other hand free to be used above the pubes to assist in fixing the uterus, and assist also in forcing open the dilating os, which can ordinarily be plainly felt through the abdominal walls." (Dr. Lombe Atthill, p. 238.)

Whether we look upon the uterus as a plastic organ to be moulded, or a muscle to be tired out, it must be equally unscientific to endeavour to force it to change its form suddenly. When it has been inverted two or three months and the process of involution has been completed, rapid reinversion, whether it be attempted by hand or by the repositor, is an operation unnecessary and unjustifiable. There is no need for such dangerous haste. It will probably be found that even gradual pressure had better be exercised intermittently rather than persistently; for the continuous forcible urging of the cup of the repositor against the fundus may result in sloughing. (Dr. J. H. Aveling, p. 245.)

IPECACUANHA IN UTERINE HEMORRHAGE AND DURING PARTURITION.—Of its utility during parturition Dr. Carriger has had great experience—an experience extending over several years. He believes that its power over uterine hemorrhage is due to its power of co-ordinating uterine action and stimulating tonic contraction. He believes that in ipecacuanha we have an oxytocic, potent and safer than ergot for both mother and child, because it stimulates the uterus into a more normal action, and at the same time facilitates dilatation of the rigid os. Dr. Carriger cites several cases in which the administration of two or three grains of the drug was followed by relaxation and dilatation of the os, great increase of the

uterine contraction, bearing down pains, and the safe and speedy termination of the labour. In one case, after the exhibition of two grains of the drug, a labour, at first believed to be impossible (owing to some contraction at the brim), was by the natural efforts alone safely terminated in less than four hours. "I might add," he says, "case upon case in which, during the past twenty years, I have thus used ipecacuanha at some period of the labour, in all of which the result has been the same—a great lessening of the sufferings of the patient, and a shortening of the duration of labour." Again he observes that in about thirty minutes after taking the ipecacuanha the restlessness, rollings and tossings of the patient cease, she becomes quiet, is sensible of the favourable change, the os dilates, the child descends, and the case is soon over, and yet the labour does not possess any of the characters of an ergotised labour, the pains being more regular, more natural, and succeeded by intervals of rest. Moreover, in a very large number of cases of rigid and undilated os, where every pain, for hours previously to its administration, had been accompanied by loud outcries, jactitation, irritability of temper, &c., this drug has rarely failed in a few minutes to cause marked change in the condition of the patient, and to conduce to the speedy termination of the labour. It is to its power of causing relaxation and dilatation of the os, that Dr. Carriger is inclined to attribute the influence of ipecacuanha over the sick stomach of pregnancy—an opinion which receives some corroboration from the fact that some obstetricians of the present day have found certain cases of obstinate vomiting to depend upon a morbid condition of that part of the womb. (Dr. J. H. Carriger, of Tennessee, p. 258.)

METRRORRHAGIA.—*Plugging the Cervical Canal.*—In those cases in which the hemorrhage persists after the entire removal of local disease, I have for many years resorted, with encouraging success, to plugging *the os uteri itself*, instead of the vagina. It occurred to me that the usual plan of filling up and distending the vagina by pieces of sponge or a handkerchief was a clumsy, painful, and inefficient mode of opposing mechanical resistance to the exit of blood from the undeveloped uterus, when its orifice could so easily be brought into sight. Acting on this idea, I have in many instances brought the cervix uteri into view, and passed inside the os two or three small pieces of cotton, tied to a piece of thread, which I wedge in firmly, covering the whole cervix with two or three larger pieces left in close contact with it on the withdrawal of the instrument. In nearly all the cases in which I have resorted to this plan, I have easily arrested the hemor-

rhage. A small piece of sponge may be used, and is more likely to remain *in situ* owing to its expansion; but as it must necessarily be very small, it is more likely to be permeated by blood. If sponge be used, great care should be taken to extract the piece passed in to the os, to which a small piece of thread should always be tied, as the os uteri might not be able to expel it alone, owing to its mode of expansion. "Plugging the os uteri in the way described is by far the most effectual way of arresting the hemorrhage which precedes and accompanies abortions, when, as sometimes occurs, it resists the usual treatment, and becomes alarming. I use dry cotton only, and no styptic or caustic solution whatever. The perchloride of iron is totally uncalled for, unnecessary; for the plugging is in the cervical canal, and the hemorrhage all but always proceeds from the uterine cavity. The treatment is as simple as corking a bottle. If the cork be well pushed in, no water (blood) can come out; the hemorrhage is arrested in a few minutes, and the patient rallies all but immediately. (Dr. H. Bennet, p. 249.)

OVARIAN MENORRHAGIA.—Of all drugs in the Pharmacopœia, I know of none which possesses such great power as the bromide of potassium in controlling ovarian menorrhagia. I ought rather to say, for that better expresses my meaning, that no other drug possesses in a like degree the power of limiting ovulation. I believe, indeed, that we may absolutely suspend the function altogether, and produce in time an atrophy of the ovary by the prolonged administration of this drug in large doses. I have seen cases again and again in which menstruation, and therefore ovulation, has been delayed for weeks from apparently no other cause than this; and I have successfully arrested and cured scores of cases of this form of menorrhagia without ever giving any kind of astringent, but merely administering the bromide of potassium. Occasionally, but by no means always, I combine with it the bromide of iron. This drug seems to me to possess a somewhat similar action; certainly it does not act in any way as the other salts of iron. It seems to favour the absorption of certain glandular swellings, which cannot be said at least of any persalt of iron. Sometimes I give the iodide of potassium, and sometimes the iodide of iron, in conjunction with the bromides. I believe that they all act very much in the same manner; but certainly none are so efficacious as the bromide of potassium. Indeed, I may say it is my sheet-anchor in these cases, and I regard it as almost, if not quite, a specific in ovarian menorrhagia. Of all the anodynes we possess, none, I think, can compare with conium as an anodyne to the

generative or sexual organs; and its influence upon the ovarian nerves is quite remarkable. I have even thought, from observations I have made, that it not only allays pain, but that it also calms vascular excitement, and so exercises a controlling and moderating influence even upon ovulation itself. Atropia or belladonna seems also to possess similar powers, but by no means to the same extent; and it has the disadvantage of causing sometimes serious constitutional disturbance even in small doses. Moreover, with an agent so efficacious, and I may add so uniform, in its results as conium, we need not multiply our resources. I may say that I always use the alkaloid *conia* in one-grain doses for a pessary, just as I prefer atropia to belladonna, because the alkaloids are cleaner in use, do not create any dirty-coloured discharge, are smaller in bulk, and are certainly not less effective. (Dr. Alfred Meadows, p. 252.)

OVARIOTOMY.—Since the adoption of Lister's antiseptic there has not been a single example of septicæmia in my cases, and the proportion of recoveries has been in the highest degree satisfactory. Thus, the antiseptic method, and next the careful avoiding of any cooling of the blood, which is so serious when the peritoneum is exposed, lead to results which were previously quite unattainable; but we must not, therefore, avoid the trouble of a very careful preparation. This care yields a rich return. Formerly, the patient was placed on the table, the tumour removed, the peritoneum cleansed as much as possible, and the wound closed. Now, after a soap-bath, the patient's abdomen is shaved, cleansed with ether, and several times washed with a five per cent. solution of carbolic acid. The whole surface, except the abdomen, is carefully covered with very warm towels, and a hot-water india-rubber bag is placed between the feet in order to restore to the mass of the blood, by warming the periphery, what is lost by laying bare a large surface of peritoneum. And our sponges and instruments are placed in a five per cent. carbolic solution, and our clean washed hands are disinfected by the same solution. The room and all liquids and objects are also warmed, if possible. Lastly, a carbonised cloud from two steam-sprays is directed upon the abdomen, and the operation begins. A dense carbolised mist covers the whole field of the operation, until, at the conclusion, a protective Lister's bandage has been applied. The operation itself has also of late undergone some modifications. The small arteries which jet in the abdominal wall are closed by catch-forceps, which renders subsequent tying generally unnecessary, as, by the time the operation is ended, a sufficient thrombus has formed.

Admitting that even a solid thrombus is not an absolute safeguard, and that only a complete soldering of the arterial cells is thoroughly trustworthy, yet the thrombus is here sufficient, because the sutures which close the opening in the abdominal wall obviate bleeding. Frequently, and especially in cases where there have been many tappings, the front wall of the ovarian cyst is strongly adherent to the abdominal wall, so that it is difficult to recognise the boundary of the peritoneum; and beginners often mistake the peritoneum for the cyst-wall, and separate the peritoneum in large surfaces from the abdominal wall. These adhesions cannot be separated without danger, because their separation often bursts the cysts, and thus the contents flow amongst the intestines. When the cyst-cells are very delicate and tense, I puncture immediately; whereas, when the cyst-cells are firm, I prefer, before puncturing, to carry the right hand, after repeatedly washing it in a five per cent. solution of carbolic acid, into the peritoneal cavity and around the whole tumour, in order to ascertain what adhesions there may be, and if they can be separated. It is then that we are first enabled to form a prognosis of the operation. Thus the length and thickness of the pedicle are ascertained. For the puncture, I always bring the patient on to her side, and when so much fluid has escaped that folds of the cyst-wall begin to form, I seize and hold them with secure forceps, enlarge the peritoneal opening by the knife, carry one hand within the opened cyst, and with the finger or the knife puncture the inner cysts so that their contents may escape through the cyst first opened. Having thus emptied the fluid part of the tumour, I then endeavour to lift out the whole mass. I have often found one or several cysts filled with such thick contents mixed with hairs, that I have been obliged to scoop them out with my hand, which is objectionable, as the hand must be thoroughly washed before it can be used to lift out the harder part of the tumour. I say "lift out," because I do rather lift out the hard tumour with the washed and carbolised hand from the bottom of the pelvis than draw it out with forceps. Now comes the separation of the various serious adhesions; I only separate slight adhesions by the fingers. The more I operate, the less I employ this mode of separation; because it readily gives rise to bleeding, which is stopped with difficulty. I tie serious adhesions with catgut and divide them with scissors. Very strong vascular adhesions I tie twice, and divide between the two ligatures; thus, it often happens that portions, one, two, or three *centrimètres* square, remain hanging from between the cyst wall and the intestines or uterus, etc., because I prefer rather to tie and divide the adhesions than to tear them

off by the hand. Nevertheless, I have forcibly torn off large pieces of cyst from the abdominal wall, from the bladder and rectum, etc., when large surfaces of the cyst wall were adherent. I have never left one single operation incomplete, and a very encouraging proportion of such bad cases have terminated favourably, so that I have not altered my opinion of late years. I have frequently in this manner perfectly overcome serious after-bleeding from dozens of small vessels, with only the supplementary compression of the abdomen with two bricks (of eight to nine pounds) which I wrapped up in paper and laid upon the Lister's gauze bandage. Neither the actual cautery nor the liquor ferri answers for such large surfaces. (Dr. Von Nussbaum, Brit. Med. Jour., Oct. 26, 1878, p. 617.)

Treatment of the Pedicle in Ovariectomy.—In the course of the year, I have greatly altered my treatment of the pedicle, and I can say that I have tried all kinds of treatment; and, before the adoption of the antiseptic method, I have found something very bad in each method. At first, I used the clamp. It has the great advantage that it affords a perfect safeguard against bleeding; but, if a pedicle be short, it causes painful traction on the uterus, and the worst is that we can never know how deep the gangrene of the pedicle may extend, for the pedicle becomes sloughy and offensive not only above the clamp, but, unfortunately, often also for some distance behind it. The part above, by the liquor ferri, or the hot iron, and in various other ways, may be protected from putridity, while the part behind the clamp is concealed and covered by the integument. If gangrene extend behind the clamp, the peritoneum of the abdominal wall is soldered to the peritoneum of the pedicle to such an extent, that at the worst below the clamp we have a deep hole between the lips of the wound, foetid and blackened by iron, from which little is absorbed if it be carefully cleaned out. But sometimes there is no such favourable soldering; the putrefying mass runs down beside the pedicle into the peritoneal cavity, and brings certain death. If, instead of the clamp, we use a small wire *écraseur*, it is possible to watch and limit the process of gangrene, as Kœberlé has done with such great success. Kœberlé has used a funnel of thin metal to surround the pedicle and constricting wire *écraseur*, to isolate them from the healthy tissues, and disinfect them by frequent injections. I have myself, after I was convinced that gangrene of the pedicle behind the clamp frequently leads to death from septicæmia, secured the pedicle with a small wire *écraseur*, and wrapped up the pedicle, the wire *écraseur*, and five or six drainage-tubes which went down to the lowest part of the pelvis, in a piece of oiled silk, and have

so isolated the pedicle. This, of course, obliges us to leave the lower angle of the wound open, and the method is no longer intraperitoneal. This modification saves all traction if the pedicle be short, for the loop of wire can be applied quite deeply in the pelvis. This mode of treatment shews also very clearly what great danger there may be in simply tying a pedicle, dividing it, and allowing it to sink into the pelvis. If the pedicle be thick and tightly compressed, immediately after dividing it one or two large thick-walled vessels spout, or after two or three hours the loop is already so loose (whether of wire, catgut, or silk) that it can be pulled off like a loose finger-ring, and violent bleeding is possible. This thinning of the pedicle illustrates the law that compression favours the absorption of fluid masses and the softening of thick tissues; besides, the wound, or division of the pedicle, allows serum to exude, and so diminishes the pedicle. Burning off the pedicle with hot iron alone, without ligature, is only successful when there are no large vessels. In all the cases where I have divided the pedicle with the cautery, I was either obliged afterwards to tie a pair of spouting vessels, or else, as a precaution, I tied a strong ligature round the whole pedicle behind the eschar. Latterly I have tied the pedicle with strong catgut in several small portions. But it is always very important not to divide the pedicle too near the ligatures, or they may slip off. This last method accords best with the antiseptic treatment. The pedicle is left quietly in the pelvis, the abdominal wound carefully closed and antiseptically bandaged. (Dr. J. N. Von Nussbaum, Prof. of Surgery in the University of Munich. *Ibid.* p. 618.)

POST-PARTUM HEMORRHAGE.—*Injection of Hot Water.*—The patient was known to be a “flooder.” Immediately following the birth of the child, violent flooding set in. I had previously given two drachms of tincture of ergot. I immediately removed the placenta, using equable pressure outside, and applied a very firm binder; but, as I anticipated, gushes of blood continued, until the woman became quite blanched. Feeling alarmed at the condition of the patient, I ordered the terrified nurse (who, I may say, was almost useless), to pour into a hand-basin some hot water, which I had previously ordered to be kept ready, and with a bath-thermometer, which I took with me, having ascertained the temperature to be 110 deg. Fahr., I injected a pint and a half into the uterus with Higginson’s syringe. To my delight, I soon found that the uterus had firmly contracted. I remained with my patient for two hours; no further hemorrhage ensued. She

states she had a better time than she ever had before. This was her fifth child. (Mr. L. Herbert Jones, *British Medical Journal*, Nov. 9, p. 690.)

SIGNIFICANCE OF THROMBOSIS OR BLOOD-CLOTTING AFTER PARTURITION.—It is well known to those—they are not, however, many at the present day—who have bled pregnant women, how readily the blood drawn coagulates, and how marked is the cupping and buffing. This is due to the excess of fibrin, resembling in this respect the blood of rheumatic fever. Proneness to clot, then, is the ordinary character of the blood in pregnancy. It is more disposed to clot in the vessels than is the blood of non-pregnant women. But the blood is not materially altered in its constitution until after two or three days have elapsed; that is until the process of involution of the uterus and of other structures which had undergone a special development to fit them for the work of parturition has set in. Then there is a rapid inpouring of effete matter into the circulation, a sudden strain thrown upon the glandular system to discharge it, and a diversion of vascular developmental activity from the pelvis to the breasts. The breasts thus perform a double duty. As well as secreting food for the infant, they act as excreting organs, and by keeping all the functions in harmonious balance. If the action of the breasts be suppressed, more work is thrown upon the other special excreting organs; and there is the greater probability of the accumulation of noxious matters in the blood. Accordingly we see that, in women who will not, or who cannot, suckle, puerperal complications, as fever, pelvic inflammations and mania are far more frequent. Now, what is the relation of these facts to the problem immediately before us, the theory of thrombosis? Why, in the first place, is thrombosis so very rare during pregnancy? At this time, the excess of fibrin in the blood is at its maximum. It is highly coagulable. Why does it not coagulate? Something is wanting to precipitate the fibrin. This something we find in the sudden irruption of effete matter, and in the product of involution, a process which does not begin until after labour. It is in excess of that quantity which the excretory organs can discharge. If this excess be very great and acting upon blood, perhaps deficient in coagulating power, the whole circulating mass is invaded: and septicæmic puerperal fever is produced. But, if the excess be in moderate proportion, it is met by quick coagulation near the seat of ingestion, and thus is prevented from seriously affecting the general mass of blood. Thrombosis thus is a conservative process. Precisely the same order of events is seen in the lymph and lymphatics which play so

active a part in absorbing the waste products of uterine involution. But the lymphatic glands further aid the conservative process of thrombosis by delaying the effete matter in its passage towards the heart, and by subjecting it probably to some kind of metamorphosis which deprives it of its noxious properties. Healthy thrombosis or phlegmasia dolens may follow the simple entry of the ordinary physiological fatty *débris* of the superfluous uterine tissue. But in a large proportion of cases the precipitating agent is something more decidedly noxious. If the uterus have contracted imperfectly, if any bits of placenta or decidua remain adherent, if clots be retained, then, decomposition or disintegration going on in the cavity of the uterus and in its sinuses, there is a direct supply of septic irritating matter. This, entering the veins and lymphatics in the broad ligaments and the internal iliacs, causes coagulation. This coagulation serves as a secondary barrier against invasion of the circulation at large; the uterus, which by firm contraction should be the first barrier, having failed. The thrombosis in the pelvic vessels is probably favoured also by the comparative slowing or retardation of the blood in them, due in some measure to the pressure of the enlarged uterus upon them. But there are many facts which point to the contact of septic matter with the blood as the immediate or efficient cause. The experiments of Gaspard, Henry Lee, and others show that the injection of septic matter into the living blood is a sufficient cause. Clinical observation adds confirmatory facts. (Dr. R. Barnes, p. 65.)

SPASMODIC DYSMENORRHOEA.—The smallest passage, a “pin-point os uteri,” as it is called, is quite enough to allow a hundred times as much blood to pass as there is any occasion for, or as offers to pass. Contraction, it is said, may be produced by swelling of the passage; but there is no special swelling of the passage. Another method of explaining the stricture is the blocking up by mucus or a blood-clot. But this kind of mechanical obstruction, even if it exists, does not induce severe dysmenorrhœa; it induces healthy uterine contractions not of a very painful kind, fitted to force on the clot or the obstructing mucus. The mechanical treatment is very successful. I know no drug that can compare with this in its direct utility. I know very few treatments that are more decidedly useful than the treatment of dysmenorrhœa by mechanical means. The mechanical treatment is very simple if carried on on the oldest of all mechanical plans recommended for the treatment of this disease—that by bougies. The treatment by bougies I recommend to you.

because it is unaccompanied by danger. The only evil result I have ever seen from it is a temporary perimetritis. It is a simple treatment, the innocence of which arises from the fact that there is no cutting, and that the instrument is not left in the womb above a few minutes at a time. It is allowed to remain till the pangs of pain which it brings on have passed. In order to effect a cure you must go up considerably above a No. 9. You must go up so as to stretch and distend the internal os uteri; and this stretching or distension of the internal os may require you, in different cases, to reach different sizes. A No. 11 is quite sufficient in many cases; in others you will go up to a 12 or 13—rarely above that. These various numbers are not all used in one day, but in successive days, or every second or third day, and generally the whole is effected in a few sittings—say from four to eight. You are not too much to expect that this treatment will cure every case. Most of the characteristic cases are, if not cured, at least greatly ameliorated. (Dr. J. Matthews Duncan, *Medical Times and Gazette*, March 1, p. 222.)

TURNING *versus* FORCEPS.—I have, as a rule, turned not only in cross births, but in almost all cases where many other practitioners would have used the long forceps, and with a result so satisfactory to my own mind as fully to justify my choice; and, as every one knows, since the introduction of chloroform, this method of dealing with obstructions and difficulty in labour, and in hastening delivery, can be much more easily and safely carried out. In malposition of the head, I never hesitate to turn when the head has not become entangled in the pelvic bones; and I have not found, when due care is exercised, that the life of the child is much jeopardised in turning. (Dr. H. S. Anderson, *Selkirk, Edinburgh Medical Journal*, June, p. 1065.)

UTERINE HEMORRHAGE FOLLOWING ABORTION.—*Subcutaneous Injection of Ether.*—A case is recorded by M. Letulle, in *La France Médicale*, of repeated uterine hemorrhage, following on abortion at the third month, in which life was apparently preserved by the subcutaneous injection of ether. Ergot of rye and vaginal pluggings had proved of only temporary service, and on the occasion of the eighth hemorrhage (thirty-two days after the first), the patient was so exhausted and anæmic that Prof. Peter suggested transfusion of blood. On the advice of M. Verneuil, however, five subcutaneous injections of sulphuric ether of five drops each were made, and at the same time two or three syringefuls of ergotine were injected. The result was instantaneous and good. Three days afterwards hemorrhage again took place, and the

patient seemed *in articulo mortis*. The vagina was plugged, and two syringefuls of ergotine and two of ether (1 gr. 50 centigs.) were injected in the space of ten minutes. Scarcely had the second injection of ether been made, when she recovered sufficiently to be able to speak, complaining of acute pains in the epigastrium. During the day two grammes of ether were injected. From that time recovery was continuous; but it was one day observed that there was complete analgesia and anæsthesia of the skin and mucous membranes and that a certain degree of dyschromatopsia was present. The ether was held to have acted as an energetic stimulant, and to have rendered the absorption of the ergotine more rapid. (M. Letulle, M. Verneuil, Glasgow Medical Journal, June, p. 482.)

UTERINE TUMOURS.—*Dilatation by Tupelo-tents.*—*Removal by Ecraseur.*—We have recently had a new kind of dilating material made known to us under the name of tupelo-tents that may, at the second sitting, be advantageously used instead of sea-tangle. This substance has been brought into notice by Dr. Sussdorff, of New York, in a paper published in the New York Medical Record of July 1877. The tents are formed from the root of the *Nyssa aquatica*, which grows in the swamps of the Southern States of America. As imported into this country, they are too short to be of much use for dilating the uterus; but Messrs. Fannin and Co., of Dublin, have procured them of the full length required. These tents swell more quickly, and in proportion to their size when dry to a greater degree, than does the sea-tangle; but the tangle can be more easily introduced in the first instance, and, from its slower and more gradual action, will probably be found less painful and safer for the patient than the other. As soon, however, as the process of dilatation has commenced, and the tissues have become softened and relaxed, the tupelo will complete it more quickly and thoroughly than the sea-tangle. If three tupelo-tents can be introduced at the second sitting, and along with them four or five pieces of No. 6 sea-tangle, the uterus will generally be found sufficiently dilated at the end of a further twenty-four hours to permit the removal of a tumour measuring from three to four inches in diameter. Having dilated the uterus and made a tumour accessible, the next step is to remove it. In the paper on uterine polypi already alluded to, the mode of removing a polypus with an écraseur is described and illustrated by a diagram; even large tumours, if prominent into the uterine cavity, may be removed in the same way. The uterus is first drawn down to the vulva,

having been seized by a strong vulsellum ; then the tumour is laid hold of either with a fine vulsellum or tenaculum, or with the "spiral instrument" described and figured in his book by Dr. McClintock, which is, indeed, nothing more or less than a long cork-screw, and the loop of a wire *écraseur* is passed round its base. In my first paper I recommended that this should be a soft iron wire ; but I now find that, for large tumours, a finely-tempered steel wire is the best, such as a piano-string, as it, though it may be compressed in passing through the os, opens again by its own elasticity when it gets into the cavity of the uterus, and is, therefore, more easily passed over the tumour, and it is, besides, firmer and stronger than the iron, and will bear a greater strain. In using an *écraseur* one of two effects will be produced. If both ends of the wire be attached to the screw, then a purely crushing movement is produced. When the screw is worked, the wire constricts the tissues till it gradually crushes its way through. If one end of the wire be attached to the screw and the other fixed, then a cutting motion is obtained combined with the crushing. This combination of cutting and crushing enables us to divide tumours that would resist and break the strongest crushing instruments ; but to obtain the combined action of cutting and crushing, the screw holding the wire must travel double the distance required in the crushing movement. With the ordinary *écraseur*, consequently, it is often necessary to stop in the middle of the operation, and readjust the wire before the operation can be completed. This might, perhaps, be obviated by using Weiss's *écraseur* which has a windlass to wind up the wire, but the instrument is very cumbrous, heavy, and inconvenient. A Dublin student, Dr. Denham, son of Mr. Denham, Ex-Master of the Rotunda Hospital, has, however, invented a simple instrument by which either a crushing or a combined crushing and cutting action can be obtained ; and by its use, what has hitherto been one of the greatest practical difficulties in cutting through the base of large sessile tumours will probably be quite overcome. The difficulty consisted in this, that to encircle a tumour of, let us say, from three to four inches in diameter, the loop of wire must be more than from nine to twelve inches in length, and if only one end of it be attached to the screw so as to give the combined cutting and crushing movement, the *écraseur* must be so long as to be unwieldy in its proportions and weakened in its powers. Denham obviates the difficulty by making one end of the wire traverse the whole length of the screw, and enabling us, this being accomplished, to make the other end, by a very simple movement, take up the action and follow the same course. (Dr. G. H. Kidd, p. 266.)

VOLSELLA.—*Use of in Gynecology.*—In cases requiring operation about the cervix, all that the surgeon has to do is to lay hold of the os uteri with a hook and draw it gently down until it fairly comes within sight; this may be effected without difficulty and without much inconvenience to the patient. Goodell says, "One word here on the subject of the volsella. Since it maintains its hold better than the tenaculum, it is to me one of the most precious instruments in my bag, amounting in value almost to a third hand. Apart from using it as above described in re-dressing or straightening out any kind of version or flexion of the womb, it subserves other useful purposes. By hooking down the cervix and holding it steady, it materially aids in the introduction of sponge-tents. For the same reason, upon the removal of the tent it renders the exploration of the uterine cavity with the finger very much easier than by the usual plan of forcing the womb down on the examining finger by suprapubic pressure, a procedure always painful, and in a fat woman very difficult of execution. By thus lowering and fixing the womb, it facilitates very materially the removal of intrauterine polypi, or the scraping away of benign or malignant growths from the cervix or the fundus. In such cases I usually apply it without the aid of the speculum, and generally seize hold of the anterior lip. In re-dressing versions, a mechanical advantage is gained by seizing hold of that lip of the cervix whose name does not correspond with that of the version. But in flexions, as one object of the traction is to stretch out the flexed side the most, that lip should be seized whose name corresponds with that of the flexion. This advice is theoretically correct: but it may not always be practicable." The instrument for daily use in the common run of cases is a simple small curved volsella, each stem ending in three short teeth. Sometimes single-pronged or two-pronged volsellæ with longer teeth may be employed; but I get the greatest amount of service from the small three-teethed variety, which it is better to have made with a catch on the handles like those on Pean's artery forceps. (Dr. Alex. R. Simpson, p. 233.)

MISCELLANEA.

ARSENIC.—*Physiological Action of.*—Fowler's solution in medicinal doses rapidly diminishes the number of red and white corpuscles in the blood of patients placed under absolutely physiological conditions. This drug is looked upon as a retrenching medicine or an inhibitory means; that is to say, medicinal doses of arsenic lower the tissue-change in the body by

reducing its expenditure. It is by the destruction of the red blood corpuscles which convey oxygen to the tissues that arsenic effects this reduction in the process of combustion, diminishing the necessity for breathing, whilst it favours the accumulation of fat in the tissues. The effects of arsenic do not continue long after its administration is stopped, and this result is explained by the rapid increase in the number of the red corpuscles which takes place after the discontinuance of the medicine. It is worthy of notice that in cases of anæmia the administration of arsenic at first increases, and then very rapidly diminishes the number of red blood corpuscles. (Drs. Elbridge, Cutler, and Bradford, Practitioner, Sept., p. 213.)

ANÆSTHETICS.—At a discussion on this subject at the Odontological Society, Mr. Hutchinson said that he had had some experience of anæsthetics, both as an administrator and also as a patient. He would not allow any one to give him chloroform, and he would not take ether unless some medical man were present to deal with the case. Hence, in regard to anæsthetics for dental operations, his opinion would be in favour of nitrous oxide; and any person experienced in its use was quite fit to administer it without having a second person present. With regard to the relative advantages of ether and chloroform, he had had a large experience of both. He believed he was one of the first to inhale ether after its first introduction into England. He took it as an experiment when he was a student, and since that time he had administered ether and chloroform very largely, and he had never lost but one patient—which was about eighteen years ago—from chloroform, and that was in a case of disease of the elbow-joint; although, he must confess, he had many alarming cases. On three or four occasions, he had patients in whose cases he had to induce artificial respiration for hours together, and for five or ten minutes, sometimes, he believed the patient was dead; but he had never lost more than one case. Mr. Salter and himself very nearly lost one patient in their own profession. It was not the nearest approach to death he had had, but it was very near to it indeed. Then came the revival of the ether practice in England. In various experiments, he had compared ether with chloroform, and his opinion was very much in favour of ether. He administered ether with a more easy mind than when administering chloroform. He preferred chloroform for very aged people and for very young infants. Under six months of age, it seemed to be very safe, and it was quicker in its effects and safer than ether. For all intervening ages, he should certainly regard himself criminally responsible if he placed any patient in danger of his

life by the use of chloroform when ether was so efficacious and so very much safer. As to whether he considered it was justifiable to administer chloroform as an anæsthetic for dental operations, Mr. Hutchinson said he did not know in what respect dental operations differed from surgical ones. If the dental patient were above sixty, he would give him chloroform; but, were it a dental or surgical operation, it certainly should not be done with one person present. Mr. Mills (anæsthetist to St. Bartholomew's Hospital) thought that in certain cases the use of chloroform was justifiable, but in ordinary dental operations he should say it was not. He should say that nitrous oxide gas was by far the most preferable, and the next to that would come ether. Chloroform was preferable for bronchitic patients, and for those where the operation was to last for two or three minutes. The administration of nitrous oxide and ether was not accompanied by so much syncope as chloroform. (Mr. Jonathan Hutchinson, Mr. Mills, Brit. Med. Jour., Nov. 16, p. 730.)

Chloramyl as an Anæsthetic.—Chloramyl, a combination of pure chloroform and nitrite of amyl, has recently been tried as an anæsthetic, at the London Hospital, by Mr. Rivington, Surgeon to the Hospital. Chloramyl was first advocated by Dr. R. Sandford, in a letter to an American journal. From experiments upon animals, he has come to the conclusion that this combination is far safer for general anæsthetic purposes than chloroform uncombined, and, "so far as tried, it seems to be fully as safe as sulphuric ether, and far more pleasant in its administration, possessing all the advantages of pure chloroform without its dangers." Mr. J. T. Clover, in reviewing Dr. Sandford's communication in the January number of the London Medical Record, stated that he made a trial of this mixture in ten cases. The anæsthesia was quickly produced, without much excitement in any case; but three suffered nausea afterwards, and two of them vomited and remained for an hour much in the same condition as if chloroform alone had been given. It appears to be similar in its action to that of a mixture of chloroform and ether; but as the vapour is less pungent, the patients generally breathe it without resistance. It was much too soon, Mr. Clover thought, to pronounce upon its relative safety. (Mr. Rivington, Dr. Sandford, and Mr. Clover, p. 289.)

Chloroform and Ethidene.—Chloroform has sometimes an unexpected and apparently capricious effect on the heart's action, the pressure being reduced with great rapidity almost to *nil*, while the pulsations are greatly retarded or even stopped. The occurrence of these sudden and unlooked for

effects on the heart's action seems to be a source of serious danger, all the more that in two instances they occurred more than a minute after chloroform had ceased to be administered and after the recovery of the blood-pressure. Under the use of ethidene, there was on no single occasion an absolute cessation either of the heart's action or of respiration, although they were sometimes very much reduced. It can, therefore, be said that, though not free from danger on the side of the heart and respiration, this agent is in a very high degree safer than chloroform. These results confirm and amplify those stated in a previous report, to the effect that ethidene does not compromise the heart as does chloroform. By the method of experimentation then employed, the effect on the blood-pressure could not be determined; and altogether the results here obtained are more exact and unequivocal. It may be added that since the last report ethidene has been given to a number of patients of all ages, with results which may be described as satisfactory. Given freely at first, it produces anæsthesia as rapidly as chloroform, and the effect could readily be kept up by comparatively small subsequent doses. The only drawback is that in some cases it produced vomiting; but it has not been determined that it does so more frequently than chloroform, over which it has the further advantage of producing less excitement and being more agreeable to the patients. So far as purely physiological tests go, ethidene has proved itself as efficient an anæsthetic as chloroform, and a much safer one. It is not asserted that this agent ought at once to replace chloroform in practice; but it is believed that a very strong case has been made out for an extensive trial of it. It appears that ethidene was first employed as an anæsthetic by Dr. Snow. He administered it in fifteen cases, with good results. (Committee of British Medical Association, Drs. Coats, Ramsay, and Mc Kendrick, of Glasgow, p. 281.)

Compound Anæsthetic Mixture.—For the last two years before and during the performance of capital operations at the Chesterfield Hospital we have almost invariably used the following anæsthetic mixture—viz., absolute alcohol, one part; chloroform, two parts; sulphuric ether, three parts. I believe it will be found to be safer than chloroform *per se*; and as a rule, quicker in its action than sulphuric ether. It is especially useful in cases of feeble pulse and partial collapse, in which, nevertheless, immediate operative interference is imperative. Dr. Charles Bell Taylor, of Nottingham, writes me as follows:—"The above mixture was first recommended by the Chloroform Committee some ten years ago. I fre-

quently use it, but don't know that it has any advantages over a few drops of chloroform applied to the ether mask when requisite during inhalation." I am inclined to form a higher opinion of it than this, and I recommend it to the notice of hospital surgeons. (Dr. John Rose, *Lancet*, May 31, p. 788.)

CELLULOID AS A BASE FOR DENTAL PURPOSES. — We have recently had an opportunity of inspecting, at the works of Messrs. J. Ritchie and Co., of Leicester Street, Leicester Square, manufacturing dentists, some highly finished specimens of work in this substance, and of reading several letters from surgeon-dentists in town and country giving their experience of the use of this material. We learn that, when worked with Gartrell's metal process with sufficient care and attention, it is not at all difficult to manipulate; that it is free from porosity or any disposition to shrink or warp; that, owing to its exceeding strength and toughness, it can be worked extremely thin (some of the uppers were quite translucent); that it is free from all tendency to taste or smell, or to setting up irritation or ulceration in tender mouths, where vulcanite might. It can be worked with platina, gold, or dental alloy. Its appearance is singularly natural and life-like. Celluloid is evidently destined to play an important part in the dentistry of the future. (*British Medical Journal*, Nov. 9, 1878, p. 691.)

CONGELATION FOR PRODUCTION OF LOCAL ANÆSTHESIA. — The simplest mode of producing local anæsthesia, suitable for some operations, is to press on the skin a bit of ice which has been dipped in salt, and to repeat the dipping several times during an application, with these slight interruptions, of two or three minutes. The same proceeding may be used as a preventive or remedy of small inflammations. If boils, for example, be thus congealed before suppuration has taken place, they will immediately be suppressed. When a freezing mixture is required for more important purposes, it is necessary that a considerable quantity of ice, which has been thoroughly pulverised and quickly mixed with about half its weight of salt, should be applied by means of a gauze net or a gutta-percha cup. Ether spray is a convenient substitute for the freezing mixture in small operations, when ice cannot be procured; but in some localities it fails from its evaporation being impeded, and in others its application is painful. The difficulty of combining pressure with it is another objection, as pressure facilitates congelation and its extension to the deeper textures. In a recent work by M. Gosselin, Professor

of Clinical Surgery in the Medical School of Paris, a minute account is given of a mode of applying a freezing mixture with pressure in evulsion of the toe-nail, for which purpose, he states, he has used it fifty-four times. As it is a mode suitable for some other operations, it may be briefly described. A small muslin bag, containing a mixture of equal parts of ice and salt, being placed on the toe and made to envelope its two sides, is then leant upon by the surgeon's hand (protected by a compress from the cold) until the congelation is effected. In concluding the subject, M. Gosselin remarks that, "Although local anæsthesia may also be obtained by the refrigeration produced by ether with Richardson's apparatus, this process requires much more time (from ten to fifteen minutes), and produces an insensibility less complete than that of the freezing mixture; and as an extremely painful operation has to be performed, we should employ the most effectual means we possess for suppressing the sensibility. (Dr. James Arnott, M. Gosselin, p. 49, and Medical Times and Gazette, March 29, p. 347.)

DIGESTIVE FERMENTS AND THEIR THERAPEUTICAL USES.—

Diastase.—Saliva has but one ferment—viz., diastase, or, as it is sometimes called, ptyalin—and its sole action is to convert starch into sugar. Saliva acts with energy on gelatinised or cooked starch, but with extreme slowness on the native unbroken starch-granules. It is important to choose the right time for giving preparations of diastase, otherwise you may obtain little or no help from them in the digestion of the starchy constituents of the meal. If you wish to get a full amount of work from a dose of malt-extract, you should administer it, like the natural saliva, with the food, or, better still, mix it with the food beforehand. The malt-extracts lend themselves exceedingly well to this latter mode of administration. They have a sweet agreeable flavour, and a teaspoonful or two may be added as a sweetener, and mixed with tea, cocoa, coffee, arrowroot, sago, or any other farinaceous dish. The only precaution to be observed is that the food should be sufficiently cooled down to be endurable in the mouth before the malt-extract is added. I have found on trial that you cannot eat or sip, even in teaspoonfuls, any substance which has a temperature above 150° Fahr., and, at this heat, diastase not only remains uninjured, but is highly active.

Pepsin.—The special ferment of the gastric juice is pepsin, and its office is to digest the albuminous and gelatigenous constituents of our food. Pepsin is only active in the presence

of an acid; and the normal acid of the stomach appears to be hydrochloric acid; other acids, however—lactic, phosphoric, citric, etc.—render pepsin active, but not so energetically as the hydrochloric. Under the influence of pepsin proteids are changed into peptone, or peptones. Pepsin preparations are especially suitable for administration by the mouth immediately after a meal. A *liquor pepsinæ* prepared by Mr. Benger, is a digestive agent of extraordinary power. A teaspoonful of this preparation in six ounces of acidulated water dissolved an ounce of chopped white of egg completely in three hours.

Proteolytic Ferment of Pancreas.—This has been named trypsin by Kühne. It differs from pepsin in requiring an alkaline (instead an acid) medium for the exercise of its powers. I found it more easy to peptonise milk by trypsin than by pepsin; on the other hand, egg-albumen was attacked more energetically by pepsin than by trypsin. Acid gastric juice is destructive of the proteolytic activity of pancreatic preparations, and it is useless to administer such preparations by the mouth, unless means be adopted to safeguard them against the action of the gastric acid. It is also plain that some of the new digestive remedies which are being sent out by eminent firms of druggists, and which are recommended expressly on the ground that they contain the combined energies of the gastric and pancreatic juices (two of these are styled respectively peptocolos and peptodyn) are compounded on erroneous principles. Pepsin and trypsin cannot possibly be combined in action. If the two ferments be present together in solution, there is no work to be got from either so long as the reaction is neutral; if you acidify, so as to waken the pepsin into activity, the trypsin is thereby rendered permanently inert; and conversely, if you quicken the trypsin into activity by adding an alkali, the pepsin loses its powers.

Pancreatic Diastase.—The pancreas is exceedingly rich in diastase. An aqueous extract of the gland (of which seven ounces represented one ounce of gland-tissue) was found to have about tenfold the starch-converting power of the best malt-extracts. We therefore possess, in pancreatic extracts, an efficient medicinal substitute for saliva in the digestion of starch.

Medicinal Equivalents of Pancreatic Juice.—The most complete, active, and convenient medicinal equivalents of pancreatic juice are the liquid extracts of the gland. These may be prepared from the pancreas of the pig, with glycerine, with water, or with brine. The glycerine extracts leaves nothing to desire on the score of activity, and it keeps perfectly; but the taste of glycerine is to some persons objectionable, and

health. The workmen employed at the various sewage outfalls, and who, though not in the sewers, breathe the effluvia arising from the settling-tanks, do not find it an unhealthy occupation." A single fact such as this is worth a great deal of theory, since it proves most conclusively one of two things—either the so-called germs of typhoid fever do not exist to any great extent in the main sewers, or else they exist in that portion of the sewer-gas which is soluble in water, and so become absorbed by the fluid contents of the drain; hence, as the men do not drink the sewer-water, they do not contract the disease. At all events it is pretty clear that the typhoid poison does not exist in any large amount in the *air* of the main sewer; and the theory which supposes that it does so, and, moreover, finds its way thence into the house-drains, and thence again into the drinking-water, and so produces the disease, is, I think, in the highest degree improbable: for why was it not absorbed in the first instance by the water in the main sewer? Is the sewer-water already saturated? If so, the amount of poison must be very great, and the comparative immunity of the sewer-men becomes simply inexplicable. Moreover, the contents of the main sewer are naturally more advanced in decomposition than those of the house-drains, and it is well known that decomposing organic matter gives off a much larger amount of gas than similar matter already decomposed; add to which the house-drain, in nine cases out of ten, is less freely ventilated than the main sewer, and, being so much smaller, it might naturally be expected that the pressure of gas would be far greater in the soil-pipe than in the sewer, and this pressure would be vastly increased if any obstruction existed whereby the discharge from the house-drain into the sewer was impeded.

Now, it so happened that during the summer of 1875, the parish authorities—actuated, no doubt, by the best of motives, but with more of zeal than discretion—insisted upon providing every house-drain in the street with a ponderous cast-iron trap. This was fitted at the unfortunate householder's expense, at the point of junction between his drain and the main sewer, and undoubtedly affords a melancholy example of the folly of extending in practice the excellent theoretical principle that every man should consume his own smoke; for the trap in question consisted of a square iron bottomless box, let into the brick-work in such a manner that the lid would open outwards from the house-drain into the sewer, and, the contents of the former having passed, would close again by its own weight—which, by the way, was no trifle. The benevolent idea was to keep the foul air of the main sewer out of the house-drain: the practical result was to prevent the fouler atmosphere of the house-drain from escaping into the sewer. At all events, such was the case

with regard to my own house, as the closets—particularly the upper one, which hitherto had been perfectly free from an offensive smell—henceforth required the frequent use of carbolic acid. The only possible effect of such a trap must be to retard the evacuation of the house-drain, and allow time for decomposition to take place therein more fully than it otherwise would do, while an accumulation of filth was collecting at the door, and waiting until reinforcements of sufficient weight arrived to effect an entrance into the main sewer. For the first few hours after it was put down this door might have worked, perhaps, with tolerable ease, but it is impossible to imagine that it could have been in its place for a week without being well-nigh set fast with rust. Be this as it may, it is quite certain that it either failed most signally in shutting out the typhoid poison, or else that poison must have been generated *de novo* on the premises, for there had been nothing like typhoid fever in the house for certainly more than twenty years, and no direct communication exists between my house-drains and those of my neighbours. The advocates of the germ theory would, of course, insist that my parish trap must have got propped open, and that some of their pet parasites, declining to become absorbed either by the water in the main sewer or by that in my house-drain, contrived, with a malevolent sagacity out of all proportion to their size, to find their foetid way up the soil-pipe, and so through the waste-pipe into the cistern; but if so, why did they wait until they were compelled to dispute their right of entrance with a massive cast-iron trap, when they could have come in without let or hindrance any time these twenty years past? How was it that, for time too long to tell, both cisterns communicated directly with the soil-pipe, and yet none of these mischievous microzymes ever thought of invasion before? I cannot but regard the theory of *contagium vivum* as highly dangerous in the hygienic point of view, since it practically excludes the possibility of diseases such as typhoid fever being generated *de novo*, for the only alternative would compel us to admit the unphilosophical doctrine of heterogenesis. Let it be once admitted that such poison can be produced by fæcal decomposition, and that such decomposition can occur within our own dwellings; and let it be moreover acknowledged that, just as in the case of the septic poison, decomposing matter is deadly, while that already putrid is rather offensive to our senses than toxically dangerous, and we shall hear much more of house-drains, and far less of sewer-gas. I do not wish, however, to convey to you the impression that I consider healthy fæcal matter capable of evolving the typhoid poison during the process of its decomposition. Were it so, I fear the disease, unhappily prevalent as it is, would be far more com-

mon. Probably you are all aware that healthy stools, like normal urine, are non-albuminous, while in certain diseased conditions either the one or the other may contain albumen. The evacuations of patients suffering from intestinal inflammation or ulceration—such, for example, as we meet with in typhoid fever or tubercular disease—are always albuminous; and possibly it is by the splitting up of this highly complex compound that the poison is generated. What may be the nature of the influence which determines the character of such decomposition, it is, of course, impossible to say; for, in the first place, the albuminoid group is a large one, and the distinctions between the various substances thus classed together are very imperfectly understood. We know, however, not only that animal and vegetable albumen differ, but that numerous modifications of this compound are met with in each sub-kingdom of nature. The albumen of egg differs from that of serum in its behaviour with reagents, and it would scarcely be too much to assert that almost every tissue of our bodies has an albumen of its own; besides which, even the same kind of albumen may undergo more than one species of decay. There is no difficulty, therefore, in accounting for the multiplicity or specificity of poisons if they are derived from such a source. Now, if we consider the special peculiarities of the fever-poisons, we shall be struck at once by their extreme potency, by the exceeding minuteness of the lethal dose in each case. In vain may we look through our list of known poisons for anything to equal it, but it is most nearly approximated by the more active members of the alkaloidal group; nor does the resemblance end here.

In man's body there exists an organ specially contrived for the purpose of eliminating an organic alkaloid. I need scarcely tell you that organ is the kidney, and the alkaloid is urea. Now, in case of disease—and, what is more remarkable, even in health, when an exclusively albuminous diet is persisted with—what we find is that the organ which normally eliminates the alkaloid, abnormally eliminates albumen; and as the quantity of the latter increases, that of the former diminishes. It is therefore probable that the alkaloidal body urea is produced by the normal decomposition of albumen, and hence, if the raw material is exported, the manufactured article cannot be supplied at the same rate.

If we turn now to the chemical properties of the organic alkaloids, and compare them with those of albumen, the resemblance is most striking. In the first place, albumen is by no means particular whether it plays the part of acid, or base, or both; and the same is true of some of the organic alkaloids. Strychnia, for example, is one of the most powerful of the vegetable bases; but it nevertheless combines readily with many

of the metallic salts, forming compounds of sparing solubility, just as albumen does. Moreover, the metallic salts which most completely precipitate albumen—such as mercuric chloride, for example—form also the most insoluble compounds with strychnia.

But we have several group tests for the alkaloids, solutions which yield precipitates with almost every one of these bodies. The most noteworthy are Sonnenschein's reagent, or the acidulated solution of sodic phosphomolybdate, which is a perfect precipitant of albumen. This solution will precipitate all the organic alkaloids with the exception of urea. Bouchardat's reagent, which consists of a solution of iodine in iodide of potassium, is another group test precipitating nearly all the alkaloids, and likewise a very complete precipitant of albumen. Precisely the same may be said of tannogallic acid; while carbazotic acid and mercuric chloride are each of them efficacious in precipitating not only albumen, but also many members of the alkaloidal group. Furthermore, the precipitates produced by carbazotic acid have in each case a distinct tendency to assume the crystalline form. Nor is the coagulation of albumen by heat an anomaly without a parallel in the alkaloidal world, for if cold water be saturated with aconitia and then filtered, we have a most undoubted alkaloid which exhibits this remarkable feature, for on boiling some of the clear filtered solution in a test-tube, we find that it becomes milky from the precipitation of a portion of the alkaloid.

When distilled with alkaline permanganate, both albumen and the alkaloids yield ammonia, while, according to Professor Odling, the putrid material which is frequently found in sewers is closely allied to the compound ammonias, and occupies a place midway between methylamine and ethylamine. I do not mean to infer from these facts that albumen actually is an alkaloid, but I think it is very likely that some of the products of its decomposition in disease may be of an alkaloidal nature, more poisonous than aconitia, and possibly, in some instances, more volatile than nicotine. In connection with this subject it is interesting to observe that the hypothesis of alkaloidal fever-poisons is to some extent supported by clinical experience, for one of the vegetable alkaloids, viz., morphia, has been frequently known to occasion a form of intermittent fever nearly allied to ague. Dr. Levinstein of Berlin has published several such cases, and the subject is also mentioned by the late Dr. Murchison in a clinical lecture published in the *Lancet* of May 10, last. Moreover, the volatile vegetable alkaloids as well as ammonia and the typhoid poison can all exist in air, can all be absorbed by water, and can all be expelled from their aqueous solution by prolonged boiling. Nor is this all, for animal charcoal, which possesses such extraordinary power of disinfecting and

sweetening the atmosphere of a sick-room by absorbing and oxydising putrid organic effluvia, is also capable of absorbing an almost unlimited quantity of gaseous ammonia, while many of the vegetable alkaloids, if mixed with it in sufficient quantity, are wholly irrecoverable by chemical means. With regard to other disinfectants and so-called germicides, it will generally be found that their antiseptic power is in direct proportion to their albuminoid affinities; and the fact that meat may be preserved from putrefaction for some time by the agency of carbolic acid, salicylic acid, thymol, and a host of other substances, admits of a ready explanation when we consider that the antiseptics in question form with albumen very insoluble, and therefore comparatively stable, compounds. Perhaps the weightiest argument which can be adduced in favour of the theory that the specific fever-poisons consist of living particles or germs, is based upon the undoubted multiplication of these poisons in the system of the affected person; but I think that even this fact admits of a different explanation: for if the poison consist of some product of albuminoid decomposition, gifted with the power of affecting those particular elements of tissue from which it was derived, it is only natural to suppose that the process thus started, possibly in some one constituent of a single cell, would be liable to spread to similar constituents of adjoining cells, in much the same manner as the ordinary inflammation of a serous membrane, for example, is known to spread. The combined products of the decomposition of the mass of tissue thus finally involved would of course supply abundant material for an enormous increase on the amount of poison originally employed. Thus the multiplication of the poison may possibly represent the waste of tissue instead of marking the extent and progress of bacterial growth. This is well illustrated in the case of specific inflammatory diseases, such as gonorrhœa, for example. A very minute portion of the gonorrhœal pus is sufficient to occasion the disease; and no one, I suppose, would dispute the fact that the poison is multiplied a thousand-fold.

Again, with regard to the period of incubation of the specific fevers, which is supposed by many to represent the time occupied by germ development, it must be borne in mind that *living* bodies do not putrefy, neither do the *living* cells of which those bodies are composed, and as in somatic death, so also in molecular death, the process of disintegration is a work of time. It may be therefore that the premonitory symptoms of these diseases set in when molecular death has proceeded to such an extent as to interfere with the functions of the affected part. If so, it would be natural to expect variation in point of time within certain limits; and clinical experience teaches that this does actually occur.

If, then, we can divest ourselves of the notion that the typhoid poison is a living entity propagating its kind by one or other of the various methods peculiar to vitalised bodies, I think there is no great difficulty in assuming the possibility of its arising *de novo*. So far as we are aware, the principal morbid change which takes place in the body of a person affected with this disease consists in a species of inflammatory affection of the intestinal mucous membrane, specially attacking the glandular structures known as Peyer's patches. Moreover, the evidence is conclusive that the evacuations from this diseased surface contain the great bulk of the poison generated in the body of the patient. I have already told you that I do not believe that the typhoid poison can result from the decomposition of healthy stools; but when those stools cease to be healthy, and become albuminous, I think it is by no means improbable that such may be the case.

Now, it so happens that, for three or four years past, one of the inmates of my house has been affected with chronic mucous diarrhoea, and I have fully satisfied myself that the faecal evacuations of this individual contain albumen. After carefully considering the evidence which I have thus briefly suggested, rather than laid before you, I am inclined to think that it is not only possible, but even probable, firstly, that the poison of typhoid fever is a soluble, and perhaps volatile body, formed on the ammonia type by the decomposition of albuminous matter derived from inflamed intestine—that it may be, in fact, a sort of animal alkaloid; and, secondly, that in the case to which I have specially directed your attention to-day the poison was most likely generated *de novo* in my own house-drain during the putrefaction of the albuminous stools already mentioned. I think also that, owing in a great measure to the obstruction existing at the point of junction of this drain with the main sewer, the gaseous products of the decomposition were prevented from escaping into the sewer, and, ascending the soil-pipe, were absorbed by the water, which, as I have already indicated, was undoubtedly the medium through which the poison was conveyed. To those among you who may hereafter be engaged in country practice, opportunities will probably be afforded of investigating this interesting and important subject, apart from the sources of fallacy which a sewered town presents; and to such I would say, if no case of typhoid fever has been previously known in the district for a long time past, and if it be quite impossible, as it often is, to trace any direct or indirect communication with an infected person, or assign any cause for the appearance of the disease, it may not be altogether unprofitable to seek for evidence of some perhaps trivial enteritic affection producing albuminous stools.—*Medical Times and Gazette*, Aug. 2, 1879, p. 118.

2.—ON THE TEMPERATURE IN RELAPSE OF TYPHOID FEVER.

By Dr. J. PEARSON IRVINE, B.A., B.Sc., Assistant Physician to Charing Cross Hospital.

Irregularity of temperature is always dangerous in typhoid fever, and that danger is not always associated with hyperpyrexia alone.

Case.—A male, aged twenty-one, was admitted into the Charing Cross Hospital in November, 1878. He was then supposed to be in the first week of an attack of typhoid, and at the time of his admission his temperature was 104° Fahr. The fever continued for twenty-one days in hospital, and then seemed to be at an end (fourth week of the disease). The defervescence during the last few days will be found in the chart below. The temperature became subnormal; the patient seemed to be doing exceedingly well; his pulse and respiration were satisfactory; his tongue was clean, he craved for food, and there was no diarrhoea,—in a word, there was every promise of speedy convalescence, when, without any warning except that given by the thermometer, a relapse set in. The temperature was $96\cdot6^{\circ}$ Fahr. on the twenty-first morning of admission. It immediately began to rise (relapse), and on the fourth morning was $104\cdot6^{\circ}$ Fahr.; on the fifth it had fallen to 103° Fahr., but in the evening was $104\cdot7^{\circ}$ Fahr. The occurrence of relapse of severe character was beyond question; the patient was evidently dangerously ill, and his pulse averaged 140. And to the eighth day there was every need to give a grave prognosis, the temperature each day persisting at about 104° Fahr., and at 3 p.m. of the eighth day reaching $105\cdot2^{\circ}$ Fahr. A very bad view was taken of the case, but a fall of fever then set in, and at 6 a.m. of the ninth day the temperature was only 100° Fahr: it had gone down five degrees in fifteen hours. This was certainly favourable, but at 9 a.m. the thermometer marked a level of 102° Fahr. On the tenth morning the temperature was once more only 100° Fahr., but the fever was “unsteady” and indefinite, and therefore (I maintain) indicative of danger. It rose also from the tenth day in a manner which pointed to danger, and on the twelfth reached $104\cdot8^{\circ}$ Fahr. It fell from this date, and on the afternoon of the fourteenth day was as low as 100° Fahr.; but the pulse was 144, and the respirations 52. On the fifteenth morning of relapse the patient died very suddenly, the temperature taken shortly before death being only $101\cdot2^{\circ}$ Fahr. He was completely conscious at this time, and immediate dissolution was not expected. Death occurred to all appearances from syncope; the patient attempted to raise himself, and fell back dead—at about 6 a.m. of the fifteenth day. On post-

mortem examination the heart was found to be flabby and its ventricles were dilated, but no perforation of the intestine could be discovered. The examination, in my absence, was made by Dr. Robert Smith, the Medical Registrar, and his excellent report states that Peyer's patches were affected as in ordinary cases of typhoid fever, and that there was nothing in the intestine to separate, by morbid appearances, relapse from primary attacks.

The interest of this case is great. The condition was from the first unfavourable, and, with the whole temperature-chart before us, its teachings are very evident, especially when we compare and contrast it with those previously given. From the fourth to the eighth day high temperatures prevailed, and on the eighth evening the fever (105° Fahr.) exceeded that of the fifth day. On the ninth morning came a satisfactory fall, but the succeeding exacerbation was too continuous, so that on the twelfth morning the temperature was 104.8° Fahr.; on the thirteenth and fourteenth days also the temperature was high. It then fell, and on the day before death was only 100° Fahr., and at the time of death 101.2° Fahr. The irregular course of the fever was indicative of danger, and it is interesting to note that this irregularity was not alone in grave prognostications. The patient was during these irregular days restless and anxious; his movements were fitful and disturbed by fine or coarse tremor; his pupils were dilated, and his eyes had a glassy look; his cheeks showed a characteristic circumscribed flushing; his tongue was deeply furrowed along the centre, and dry and fleshy along the edges, and his pulse was comparatively slow; and in spite of the restlessness there was a constant dangerous tendency to coma-vigil. With the fall of the temperature to 100° Fahr. (the day before death) the respirations increased to 52, and the pulse rose to 145, and indicated failing heart-power. So the irregularities of the fever, as accurately measured by the thermometer, confirmed the teachings of danger which the unaided hand and eye had detected.—*Medical Times and Gazette*, April 26, 1879, p. 449.

3.—ON THE VALUE OF ALCOHOL IN FEVER.

By Dr. H. MACNAUGHTON JONES, Cork.

Dr. Jones said that he meant his remarks to be essentially practical, giving rather the results of his individual bedside experience than broaching any views or theories. Independently of private cases, he had treated in the Cork Fever Hospital, from January 1873 to June 30th, 1879, 899 cases of typhus, typhoid, and simple continued fever. It was in 1873 that he first determined to watch carefully the effects of digitalis in

fever, and learned to abandon the too generous use of alcohol; nay more, to administer it only in a certain percentage of cases in which the indications for its employment were clear, and when the responsibility of withholding stimulants would be infinitely greater than that of administering them. He had since been most careful in ordering brandy or wine of any kind in fever. In 1875, he published, in the Dublin Monthly Journal of Medical Science, a table of 310 cases of fever; of these, 220 got no stimulants, 58 had claret alone, 33 had brandy. Of this number, 110 were typhus patients, of whom 26 had stimulants. The mortality in the typhus cases was 8 per cent.; in all other cases $3\frac{1}{2}$ per cent. The brandy in all the fatal cases was commenced from the first to the fifth day. In that period he had two deaths from typhoid fever; both patients had stimulants from an early date in the attack. He had divided the patients treated since the publication of this table under two heads: Total patients treated, with percentage of deaths; and total patients treated since January 1877. He had not an accurate record of the patients who had stimulants from February 1875 to January 1877, as the hospital wine-book was unfortunately mislaid. But the average number of patients who received stimulants was about the same as that from January 1873 to February 1875, during which time 30 per cent. of the cases had stimulants at some period of the fever. From January 1877 to June 30th, 1879, 123 out of 340, or 36·17 per cent., had stimulants. In typhus fever, stimulants were given during the first period to 23·6 per cent. of the cases, and in the latter to 41·37 per cent. In typhoid fever, of 107 cases, 22 had brandy and wine, 27 claret alone. The total percentage taking brandy was 20·56; the total percentage taking claret, 25·23; the deaths of the former were 36·36 per cent.; of the latter, 7·40 per cent. Of 117 cases of simple continued fever, 5·12 per cent. were given brandy and 5·12 per cent. claret. There were no deaths. In these cases, temperature-charts of the disease were kept. Sphygmographic tracings were taken of several during the illness. In summing up his conviction, from bedside experience, of the value of alcohol in fever, Dr. Jones said that it is a most valuable therapeutic agent in both typhus and typhoid fever. A large percentage of cases not only do not require it, but its administration is apt to lead to complications. It is impossible to lay down rules as to the stage of the fever in which it may be indicated, as this indication depends rather on the type of the fever than on its stage. But the time to watch for its administration in ordinary cases is from the eighth to the twelfth day. Early administration of stimulants in fever is injudicious. He had little faith in their early employment *preventing* an adynamic condition, and he had rarely seen

them have a good effect in the early stages of the fever of habitual drinkers. He was inclined to think that it is a dangerous fallacy to regard them as essential in such cases. Of the two, he had more often seen hard-drinkers recover without stimulants than with them. Alcohol, in his experience, had little effect on the temperature of fever. He generally took, as his tests, the age of patients, the condition of the heart, the pulse, tongue, and the head symptoms. Young patients, as a rule, do well without stimulants. A feeble, irregularly acting heart, with weakened first sound; a compressible and rapid pulse; a tongue keeping fairly moist; the absence of violent head-symptoms; encouraged him in their continuance and use. He believed that we possess in alcohol a supporting food in those typhoid states when assimilation is difficult, and he had many times seen lives saved in fever, sustained for days by brandy and milk alone, when everything else had been rejected. The practices he conceived most to be deplored in the administration of alcohol were: (*a*) the indiscriminate employment in the earlier stages of fever; (*b*) the rash continuance and the increase of the quantity used, when the symptoms show that it is acting injuriously and is of no service.—*British Medical Journal*, August 23, 1879, p. 284.

4.—ON THE RECENT EPIDEMIC OF ENTERIC FEVER IN THE CATERHAM VALLEY.

By the EDITOR of the BRITISH MEDICAL JOURNAL.

The official report on the remarkable epidemic of enteric fever at Caterham and Redhill, which has already been several times referred to in these columns, has just been issued by the Local Government Board. We are now enabled for the first time to judge of the value of the evidence upon which Dr. Thorne came to the conclusion that the epidemic was occasioned by the pollution of the water-supply of the Caterham Company by the evacuations of one of their workmen, and it must be confessed that Dr. Thorne has perfectly succeeded in proving his case. Indeed, the epidemic partakes of the character of a gigantic but involuntary chemical experiment, since nothing can be clearer than the relation of the pollution of the water and the outbreak of the fever. Although the main facts have been already reported by us, it will be convenient that we should again trace the course of the inquiry, so that the successive steps which led to the discovery of the causation of the epidemic may be the more clearly seen. Incidentally, the method of exclusion which appears to be followed by the medical officers of the Local Government Board in investigating epidemics, and which achieved such remarkable results

in the Marylebone typhoid fever epidemic of 1873, the North London diphtheria outbreak of last year, and in many other similar cases, will come out in strong relief.

On the 4th of February last, representations were made to the Local Government Board that a sudden outbreak of typhoid fever (supposed by the medical man who gave the information to have been due to polluted water) had occurred at Caterham. The next day, Dr. Thorne commenced his inquiry, finding that up to that date forty-seven cases of the disease had occurred within a fortnight. The cases were found to be spread over a very wide area, and the houses attacked belonged to no special class, both rich and poor having suffered. It was at once apparent that the disease could not have been conveyed to the affected houses by means of any general system of sewers common to the district, for by far the majority of the houses were found to drain into separate cesspools excavated into the chalk. There was also no possibility that there had been any common cause of infection in connection with the prevailing means of excrement-disposal, because there was nothing in common with regard to such disposal. Most of the houses in which the disease had appeared were provided with water-closets, a considerable number of which were apparently well trapped and ventilated. Others had closets which were fitted with a trap and pan and emptied into cesspools, and though these closets were at times a source of nuisance, owing to the absence of any flushing apparatus, yet they were all situated out of doors. Others, again, were provided with common privies, and a few with earth-closets. The possibility of infection having been communicated by means of a milk-supply was next inquired into, and it was ascertained that thirty-three of the houses affected received their milk from at least five different and completely independent dairies, and that at the remaining two private cows were kept. It was also evident that personal infection could not in any way have led to the outbreak. Further, there was no history of any recent prevalence of enteric fever in Caterham. It was stated that the locality for some years past had been remarkably free from the disease, and during the twelve months preceding the outbreak only one isolated (imported) case could be heard of.

With regard to the water-supply, it was ascertained that out of a total of 558 houses in Caterham, 419 were provided with water from the mains of the Caterham Waterworks Company, the remaining 139 deriving their supply from local wells or from rain-water tanks and barrels. Of the 47 persons attacked during the fortnight from January 19th to February 2nd, 45 resided in houses where the water of the Caterham Waterworks Company was in use, a circumstance which, having regard to the other

points already adverted to, indicated a likelihood that this water had been the means by which the infection had been conveyed. This view received confirmation when it was ascertained that the two remaining patients, who resided on premises where private wells were in use, had, owing to the nature of their employment, not only been in the habit of spending the day at houses to which the Company's water was laid on, but had admittedly used this water. Moreover, at the Caterham Lunatic Asylum, with two thousand inmates, and at the Caterham Barracks, where five hundred soldiers were quartered, the water-supply for both of which was a deep well belonging to the Metropolitan Asylums Board, not a single case had occurred or did occur during the whole course of the epidemic.

Whilst these inquiries were going on, information was received of a similar epidemic at Redhill, which is about eight miles from Caterham, and differs from it in several material respects, lying on the lower greensand instead of on the chalk, and being provided with a modern and efficient system of sewers. Here the epidemic had commenced at the same time as that at Caterham, and it was found that, during the first fortnight of the outbreak, 91 out of the 96 person attacked drew their water from the mains of the Caterham Company, which also supplied Redhill. Reigate town, which forms part of the sanitary district in which Redhill is situated, but which has a different water-supply, entirely escaped; and this and other circumstances detailed by Dr. Thorne point clearly enough to the water supplied by the Caterham Company as the source of the mischief.

This being so, the next step was to discover in what way the poison of enteric fever could have had access to the water. Here Dr. Thorne met at first with some difficulty, until he made inquiry as to any illness amongst the workmen who were engaged in making an adit from one of the two existing wells of the Company up to a new bore which was being sunk into the lower greensand about ninety feet distant. It was then found that one of the men had been ailing whilst at his work, and a lengthened examination of him made it apparent that during the time he was engaged in the adit he was suffering from a mild form of enteric fever.

For the purposes of his argument, Dr. Thorne finds it necessary to give in considerable detail the symptoms of this man's illness, especially as to his copious diarrhoea; but it will be sufficient for us to say that there cannot be a doubt that the man's evacuations passed into the water in the adit, and were thence distributed all over the Company's system. And that this is so is amply born out by the occurrences which followed. If the stools of this man—who began to feel ill on January 5th, and continued at work till the 20th—could by any means have

found their way into the water of the well in which he had been working, and, being enteric fever stools, could thus have led to the development of the poison of that disease in the well, the effect on the water-consumers ought to have been noticed within from about ten to fourteen days after the date when the diarrhoea first came on. And, this in effect, is precisely what did take place, the epidemic having commenced on January 19th and 20th in Catherham and Redhill respectively.

Now we know from ample experience that enteric fever is produced, and produced with the maximum of certainty, when the specific evacuations of that disease are consumed by a population. Again, it is a matter of experience that, where enteric fever has been conveyed through water, about a fortnight has to elapse between the distribution of the water and the occurrence of the disease among the community served by it. But a fortnight after January 5th is the very day when the first case of fever occurred, and during the fortnight following upon the period January 5th to 20th—*i. e.*, from January 19th to February 3rd—the disease became widely spread throughout Catherham and Redhill; the distribution of the fever being limited, as has been already shown, to houses supplied with the water of the Catherham Company.

Hitherto the course of the epidemic, during the first fortnight of its progress, has alone been dealt with. No diminution in the epidemic was apparent until about the 11th and 12th of February. Up to about the 20th February, the disease continued exclusively to attack persons residing in houses to which the Catherham Company's water was laid on, or it selected from amongst those who habitually used another supply, those who had partaken of the Company's water occasionally, either whilst at their work or during a visit to houses to which it was laid on. After this time, however, the disease became, as might have been predicted, somewhat more diffused, and several of the later cases occurred in houses where there had been previous ones. Up to the end of February, the total number of attacks concerning which information had been obtained, and including 13 cases in which the dates of attack could not with any accuracy be ascertained, was 305; of these, 224, 9 of which terminated fatally, occurred in the town of Redhill, and 81, including 5 fatal cases, at Catherham.

As the area of the Catherham Company's water-service includes other places than those hitherto named, inquiry was made in these places as to circumstances that should confirm or disprove the conclusions so far arrived at. The information thus obtained, though not at all as intelligible as that relating to Catherham and Redhill, was, however, found to be in harmony with it when all the facts were ascertained. Dr. Thorne

goes at great length into the circumstances of the attacks at these places as still further proving his case; but it would be supererogation on our part to follow him. Our readers will probably agree with us in thinking that he has already more than succeeded in accounting for the epidemic. That a total of 352 cases and 21 deaths should be a consequence of the act of one man is, it must be confessed, not a very encouraging subject for thought; but there is some comfort to be got from the fact that the exciting cause of the epidemic was a temporary and unforeseen one, and that every possible remedial measure was at once taken.

It remains to be stated that throughout the disease was typical enteric fever, the patients exhibiting the characteristic rose-spots and diarrhoea, and some of them suffering from severe pulmonary and intestinal complications, the latter including perforation of the bowels, which in four cases was the immediate cause of death. Notwithstanding this, however, by far the majority of the cases were of an exceptionally mild character; and, although, when subjected to examination, they were ascertained to be cases of genuine enteric fever, it is certain that, had it not been for the prevailing epidemic, no medical advice would in many cases have been sought, and some of the patients would not even have taken to their beds. The largest number of attacks fell upon children; and, amongst adults, women were more frequently attacked than men. This is only what might have been expected in an epidemic in which water is the vehicle by which the infection is conveyed, since children drink much more water than adults, and, amongst the latter, men as a rule drink less than women.—*British Medical Journal*, May 3, 1879, p. 669.

5.—NOTES ON SOME OF THE LESS COMMON FORMS OF SCARLATINA, WITH CASES.

By Dr. F. ROYSTON FAIRBANK, Surgeon to the General Hospital, Doncaster.

Scarlatina Sine Eruptione.—Case 1.—During the prevalence of scarlet fever in a severe form, on May 3rd a youth, aged sixteen, was taken ill, and had severe rigors. The bowels were much purged. On the 5th the tongue was dry and brown, throat painful, tonsils swollen and deep red, covered with thick glairy mucus. There was copious discharge of mucus from the nose. He had a very anxious look about his face. Pulse 130, small and weak. The next day, May 6th, general condition the same; delirious all day; kept constantly trying to get out of bed. He was evidently in a very critical condition. I had formed the opinion that he was suffering from scarlatina. I

ordered compound tincture of cinchona, tincture of digitalis, and solution of acetate of ammonia. The next day he was more composed, and looked less anxious. The tongue was moist, red round the edge, and furred in the centre; tonsils less swollen; the nasal discharge purulent; purging abated; pulse fuller and slower. On the 9th the tongue resembled a strawberry, as in scarlatina; tonsils normal in size, but with the soft palate covered with superficial ulceration. The case did well, and there was a good recovery. There was no desquamation.

Case 2.—On May 9th, in the same house, the servant girl, aged fourteen, suffered from high fever. The throat and soft palate were red and painful. There was no rash, and no desquamation.

Case 3.—On May 9th, in the same house, the daughter, aged eight, suffered from high fever. The throat was red, as in scarlet fever. The skin was red all over, as from slight erythema rather than scarlatina.

Case 4.—On May 10th, in the same house, an infant had a well-marked attack of ordinary scarlatina, with rash freely out.

Case 5.—On May 15th, in the same house, the son, aged six years, had a sharp attack of well-marked scarlatina. The rash was thickly out; the soft palate and tonsils were of a deep-red colour and dotted with ulcers. There was copious desquamation.

I could give other instances similar to these. There was no doubt about there being no rash in the earlier cases. I directed special attention to them. Sir William Jenner says of “scarlatina sine eruptione”: “I doubt the existence of this. There are many cases where the eruption is very slight indeed, just a scarlet blush lasting a few hours, and perhaps there are cases without any rash.” On the other hand, Bretonneau says:—“Although the cutaneous eruption which usually accompanies scarlatinal angina presents a character which is easily discerned, it cannot be doubted that the phlegmasia of the pharynx proper to scarlatina often exists without being accompanied by the other symptoms of scarlatina.” Graves, Trousseau, Wunderlich, and others express opinions similar to Bretonneau’s.

Case 6.—Here is another case which occurred during the same epidemic as the others. Mary T—, aged twenty, an unhealthy, anæmic girl, who suffered from leucorrhœa and repeated abscesses of the labia at the menstrual periods, was taken severely ill on June 12th with sore-throat and fever. There was diarrhœa with severe headache and delirium. The tongue was at first heavily furred, and then presented the strawberry appearance of scarlatina. The throat, tonsils, and soft palate were swollen, and of a deep-red colour. There were for a short time a few red punctuations under the skin, but from first to last there was no rash. There was free desquamation

subsequently. She made a good recovery. This girl was very ill, and the diagnosis lay between brain-mischief and scarlatina, which was epidemic at the time. I expressed the opinion that it was the latter, as it afterwards proved to be. Trousseau mentions several cases similar to this; in most of them the rash came out, but very late. In many cases where there is rash the illness is comparatively slight, but occasionally it is severe, as in cases 1 and 6 of the foregoing series, where there is great danger of death from adynamia. In some epidemics there is a considerable percentage of cases without rash, while in others there are few or none. There are cases where death takes place before the rash has time to come out, the patients being knocked down by the strength of the poison, but these belong to a different class.

Scarlatina, with affections of the joints and of the heart as a complication.—Case 1.—A girl aged seven years. The rash came out freely. The tonsils and throat were red and swollen, but not ulcerated. On the tenth day of the illness the wrists, knees, and ankles were swollen and painful. The shoulders followed, and there was severe pain in the back of the neck. The urine was at the time scanty and high coloured. The pain and swelling lasted three or four days, and subsided. There was subsequent great debility, but the case made a good recovery.

Case 2.—A girl, aged twelve years. The scarlatina rash came out freely. On the third day of illness the back and upper part of the front of the chest and the legs were covered with miliary vesicles in addition to the scarlet rash. On the sixth day there was free desquamation on the hands, back, and front of the chest, and on the legs the vesicles were opaque. The ankles and feet were stiff and swollen; so also was the right elbow. There was pain of a rheumatic character. The urine was clear, of sp. gr. 1015; it deposited albumen to $\frac{1}{10}$. There were also transparent tubular casts and epithelium. The stiffness and pain in the joints lasted three or four days, and subsided. Subsequently the nails, in the course of their growth, showed a furrow marking the date of the illness.

Case 3.—A younger sister of the last patient. The scarlatina rash came out freely over the body. There were no vesicles. The throat was very sore and red, but the tonsils were not swollen or ulcerated. On the fifteenth day of illness desquamation was proceeding. Most of the joints were painful and swollen, and the finger-ends and toes appeared likely to slough; indeed there appeared to be pus under the skin, but it was subsequently absorbed. The urine was scanty, and there was a heavy deposit of salmon-coloured lithates. The sounds of the heart were not altered.

Case 4.—A girl, aged sixteen years. The scarlatina rash came out freely. The tonsils and soft palate were of a deep red colour, covered with white pultaceous matter. There was no ulceration. On the sixth day of illness the wrists were painful and swollen. The urine was of sp. gr. 1030, and there was no albumen. The pain and swelling of the joints lasted four days, and then subsided. There was a good recovery.

Trousseau says of this class of cases:—"Rheumatism complicates one-third of all cases of scarlet fever in adults during the acute stage, but is overlooked owing to its being confined to two or three joints, usually the wrists, and also to its being very slight. It is important to know this, for acute affections of the joints—general arthritis,—pericarditis, and endocarditis frequently occur during the course of the disease. They seem to be of the nature of rheumatism. Generally speaking, scarlatinous rheumatism attacks the joints in the first instance, and then the serous membranes of the heart and pleura; but sometimes, like pure rheumatism, it seizes on the thoracic organs at the first brunt without touching the articulations. The coincidence of rheumatism with scarlatina was nevertheless a generally forgotten fact; and consequently for several years past I have been constantly insisting upon it in my lectures."

Hebra expresses himself in a similar manner. Dr Richardson, speaking of cases of this class, says:—"There was the fact, in spite of any hypothesis to the contrary, that two diseases may exist in the same body at the same time." But I would ask, Is rheumatism a *disease* to be spoken of in the same sense as scarlatina? Niemeyer considers these so-called rheumatic symptoms a localisation of the action of the poison of scarlatina in the parts affected as much as it is in the throat and kidney affections.

Sir Thomas Watson considers them due to the retention in the blood of a poisonous excrement, by the default of the principal emunctories, and especially the kidneys. Dr. West says:—"As in other blood diseases, so in scarlet fever, we meet now and then with secondary inflammation of the joints, which may even go on to the formation of pus."

Case 5.—The worst case I ever saw—and it is one which well illustrates some of the foregoing observations—was as follows:—Clara H—, aged ten, began to be ill with scarlatina on July 6th. There was an epidemic at the time of unusual severity, the weather being intensely hot. Her brother was ill with the fever in the same room, and died with diffuse suppuration in the cellular tissue of the neck. With the girl the eruption came out on the second day of illness; it was confined to a single patch on the abdomen about six inches in diameter,

and was very dark in colour. The tonsils were much swollen and of a dark red colour, but not ulcerated; pulse small; urine scanty and high-coloured. On the second day of eruption—the fourth of the illness—the wrists and knees became painful and greatly swollen. The following day symptoms of pericarditis set in with great violence, attended with continuous delirium. The heart beat so tumultuously that the sounds could not be distinguished. The pulse at the wrist could not be counted. I gave tincture of digitalis and tincture of opium, of each five minims, with ten grains of nitrate of potassium, every two hours, until easier. The next morning she was much better every way; the heart was more tranquil, the sounds were distinct, accompanied by a “to-and-fro” friction-sound; the pulse was 125, and of fair strength; the joints were less painful and swollen; urine more plentiful; delirium occasional only. She continued the medicine at longer intervals for three days, and was then put on tonics. Three months afterwards she had regained her strength; the sounds of her heart were then clear, and there was no friction-sound.

Dr. George Budd, of King's College, has recorded a case of this kind, in which, after death, recent lymph was found on the surface of the heart, and a few ounces of serous fluid in the pericardium. No abnormal sound had been heard before death, though the action of the heart was feeble and irregular.

I have a long series of cases of this so-called rheumatic complication, and nearly all of them were children. In all, the illness was of a severe character; the fever was high; the rash deep-coloured; the tonsils deeply congested or ulcerated; the urine albuminous, or depositing salmon-coloured lithates. The nails have been subsequently furrowed, corresponding with the date of illness. I am, therefore, inclined to the belief, as expressed by Sir Thomas Watson, that the complication results from the retention of poisonous excrement in the blood, and that it is a form of septicæmia. In some epidemics the majority of patients are affected with these so-called rheumatic symptoms, so that they give quite a character to the epidemic. In other epidemics there are no cases at all. Like the other complications of scarlet fever they appear to depend more on the character of the epidemic than on the individual peculiarities of the patients.

The milder cases do not require any special treatment; the symptoms subside in three or four days; but the more severe forms will require appropriate treatment, the possibility of ankylosis or suppuration being specially borne in mind, and so also the possibility of mischief about the heart.—*Lancet*, Aug. 16, 1879, p. 237.

6.—ACCOUNT OF AN EPIDEMIC OF SURGICAL SCARLATINA OCCURRING IN GUY'S HOSPITAL IN 1878.

By H. G. HOWSE, Esq., M.S., Surgeon to Guy's Hospital.

On March 15th, 1878, my colleague, Mr. Jacobson, operated by osteotomy upon a child suffering from extreme rickets. This operation was followed almost immediately by a moderately high temperature, varying between 100° and 101° , and after three days by the appearance of a roseoloid rash, accompanied by sore throat and a strawberry tongue. The operation was performed under the carbolic spray, and all the usual well-recognised antiseptic details were duly carried out. Medical opinions were called in, but, as is not unusual in such cases, they were divided between the diagnosis of scarlatina and the roseoloid rash of septicæmia. To be upon the safe side, however, the child was removed to a private room at the end of the ward, where she was attended separately for six days, the antiseptic dressings being continued assiduously during the whole period. The rash speedily subsided, the temperature fell, and as great doubt had been expressed about its being a case of scarlatina at all, leave was given to bring the child back into its old bed.

On April 1st abundant albumen was noted in the urine, and the skin subsequently desquamated. The case ultimately did well, though undoubtedly retarded by this intercurrent affection. It is to be noted that the same operation had been successfully performed three months before upon the other leg, and that no similar condition of fever resulted from that operation.

On April 5th my dresser, Mr. Stanley Taylor, operated under my supervision for the removal of a lipoma. The temperature rose the day after the operation to 101° , and remained at that level till April 8th, when it rose suddenly to 103.6° , and the patient developed a rose rash over the body and complained of sore throat.

Being absent from the ward for a day or two about this time I did not immediately hear of the symptoms which had developed themselves, but when I did so separated the patient by ordering her removal to the farther (nearly empty) division of the ward, apart from all the other cases, giving instructions to the dresser, Mr. Taylor, to dress this wound last of all in the day, while we waited and watched to see whether any fresh cases would arise in patients more recently operated on. I was at this time in ignorance of any similar case having occurred in the ward.

On April 9th I excised the elbow-joint of a girl æt. 16, suffering from pulpy disease. On the 10th her temperature began to increase, and on the 11th rose to 105.8° . Towards the evening

she developed a roseoloid eruption on the skin. This case was placed with the other with the same instructions.

On April 12th I excised a fibroid bursa patellæ from a woman, æt. 29. The day following the operation the temperature was 99°, but the next day it rose to 100°, and on the evening of the 15th she had rigors and headache. On the morning of the 16th the temperature was 102·5°, and she had a roseoloid rash over the chest and face.

Awaking now to the fact that something was going wrong in the ward, I instituted strict inquiries as to the possible origin of the outbreak, and then ascertained the facts already detailed about Case 1. At the time that I operated upon my first case it is probable that this child was still suffering from albuminuria, and that desquamation had commenced, which process, indeed, was still going on. To cut short the epidemic, therefore, I had all four cases removed on April 16th to two private wards, where they were attended by separate nurses, and where Mr. Jacobson undertook to see after them, putting them in charge of one of his own special dressers; and I further resolved that for a week or ten days I would not do any more operations in this ward unless in a case of emergency; in fact, a fortnight elapsed before the next operation was done in Astley Cooper Ward.

These measures were successful. The epidemic ceased.

[Several other cases occurred followed by the same kind of rash. Mr. Howse then proceeds to say:—]

In the last volume of "Guy's Hospital Reports" (vol. xxiii, p. 318), in a note to Mr. Davy's paper on "Antiseptic Surgery and Pyæmia," I said that "not one of the least benefits which Mr. Lister's system of dressing wounds had conferred on surgeons" would be the possibility of separating at any rate one group of cases from the domain of septicæmia, and proving their exanthematous origin. For many years the existence of a rose rash at the commencement of many septicæmic cases has been noticed by surgeons and obstetric physicians. Thus, in a paper published in the "Transactions of the Obstetrical Society" (vol. xii), Dr. Braxton Hicks has called attention to the rose rashes occurring in puerperal fever, and has connected them with sources of infection from scarlatinal cases, &c. But these cases nearly all terminated in the ordinary symptoms of puerperal fever, and the object of this very important paper was mainly to show how ordinary puerperal fever arose from sources of infection of zymotic origin. Thus, he has proved that not only the contagium of scarlatina, but also those of measles, diphtheria, erysipelas, typhus, and typhoid, gave rise to puerperal fever. Nevertheless, if his cases are looked into carefully, it will be seen that only in those in which there is proof of

the existence of the scarlatinal poison, has the definite rose-rash preceded the puerperal fever. This proof of the presence of scarlatinal poison was not always obtained prior to the outbreak of the puerperal fever, but in nearly all those cases in which it was difficult to prove its presence *before*, some indication of its existence *after* was afforded by an outbreak of the disease either in the children, or in some other persons living under the same roof.

Again, in the older surgical writings, it is very common to read in the descriptions of surgical fever resulting from operations and injuries, that the patient sank into a "typhoid" condition, or became affected with "typhus." It is probable that surgeons of this date, before zymotic affections had been studied with anything like the attention that has since been devoted to them, long before "common continued fever" had been differentiated out into the two affections now recognised as existing, really believed that this state was identical with the fevers the names of which they adopted. And this is the more likely, as in campaigns where active surgery was principally called into play, epidemics of typhus, especially when the troops were exposed to great hardships with deficient supplies of food, and insufficient attention to sanitary arrangements, did really prevail. Such epidemics, especially when morbid anatomy was not studied as at present, and when the disease now known as septicæmia had not yet been recognised, would tend to confuse the minds of medical observers, and incline them to associate together things which were really distinct.

But at the present day it is probable that we have gone to the opposite extreme. Now that septicæmia has had so much study bestowed upon it, the tendency is to associate all states of fever occurring after operations, &c., under this term. This is scarcely more correct than our forefathers' generalisations. And hence we read expressions such as these, "the rose-rash of septicæmia."

Is there such a thing as a rose-rash in a typical case of septicæmia? I will not venture to affirm that there is *not*, because the proof of a negation such as this must always be next to impossible. But I will venture to affirm that the more these cases are studied, especially when the disease occurs in groups of cases, and in patients that have been dressed antiseptically, the deeper will become the conviction that they have little in common with true septicæmia, and that they all originate in the first place in a true scarlatinal infection.

Few persons who read the notes of the above-described epidemic will doubt that we were here dealing with a true scarlatinal poison. It is true that the cases in one or two

small particulars appeared to differ from what may be regarded as a *typical* case of scarlatina. But cases of scarlatina, even occurring in the same epidemic, differ so much amongst themselves that this objection becomes of small importance. It is probable, too, that all these cases originated from the child who first became affected in the ward. And in relation to this, it is interesting to note that all the cases subsequently occurring were operated upon for affections which did not preclude them, prior to the operation, from walking about the ward, and thus placing themselves in close propinquity to the other patients. The child who was the first subject of the affection was rather a favourite, and thus was one who was very likely to have been caressed by, and become a centre of infection to, the other patients. I should suppose, therefore, that each of the patients who subsequently suffered from the fever derived the germs of the affection by personal contact with this child. This is rendered almost certain when we remember that nobody else in the ward caught it. It is, indeed, remarkable that none of the patients operated upon previously (of whom there were many in the ward) suffered from the disease. The only explanation that can suggest itself is, that being confined to their beds they were out of range of the infecting distance of the fever poison. Nor, indeed, can it be maintained that the disease was propagated by surgeon, dressers, or nurses passing from patient to patient. Certainly not by surgeon or dressers, for, during the period, I performed frequent operations in the other wards of the hospital, and my dressers attended to them, but not a single case occurred elsewhere. Nor is it likely that the nurses were a source of infection, though this is more difficult to prove; but if they were so, it is curious, to say the least, that they did not propagate the disease amongst some of the other patients lying in bed, who were the subjects of much more serious complaints than those actually infected.

The position, then, which I wish to take up as regards the relation of these cases to septicæmia is that, without the anti-septic dressings, the disease would speedily have become undistinguishable from septicæmia. It is well known that a high temperature, within certain limits, promotes decomposition. Scarlatina causes a high temperature. If during its prevalence the patient has an open wound, as yet unprotected even by granulation tissue, the discharges in contact with it will speedily decompose, and; being absorbed, will cause definite septicæmic symptoms. This process of absorption will be favoured by the fact that healthy granulation tissue will rarely form in adults at a constant body-heat of above 102°. And if the fever should cause a body-heat of from 104° to 105° granulation tissue *already formed* dies, and a process of slough-

ing sets in, which in its turn favours the absorption of septicæmic material; but, even short of the process of actual sloughing, if we suppose a person to have absorbed the germs of a scarlatina shortly before an operation is performed, it is easily understood how, if the temperature only rise to 101° or 102° , no protective granulation tissue will form about the wound, and thus it will remain in the most favourable condition for the absorption of the poisonous materials resulting from decomposition. Hence a scarlatina affecting a person with an open wound is apt to pass into a condition of septicæmia, and then, if the patient dies, as he very probably may, and a post mortem be held on him, the death will be certified to as one from septicæmia, without any reference to the action of the scarlatinal poison which was its original cause. This is because the signs of death from pyæmia or septicæmia in the cadaver are evident and well recognised, whereas those from scarlatina are dubious and vague. These facts I have before mentioned in the note to page 318 of the last volume of these Reports. Such cases are not by any means of very rare occurrence. Thus, Mr. Bryant has recorded one in vol. xxii, p. 328, of these Reports, and the late Mr. Hilton used to mention a case in his "Clinical Lectures" in which he had operated upon a small cyst in a young lady's neck. Twenty-four hours after the operation she developed a high temperature and a roseoloid rash over the body; the wound took on a sloughy character, and in the course of three or four days she passed into a septicæmic condition, and speedily died. Mr. Hilton used to employ this case as an argument against doing even the most trifling operations without giving the most guarded prognosis beforehand. The records of death from septicæmia would show many others, if the clinical reports of the cases during life were also carefully searched, but Mr. Hilton's observations would lose in the present day much of their force, with the greater knowledge which we now possess as regards the value of antiseptic precautions.

My contention, therefore, is, that we are able, with our antiseptic dressings, to prove as conclusively as anything can be proved in surgery and medicine (where so many disturbing factors exist) that these cases are really scarlatina cases, and that the septicæmia is merely a secondary condition. For the study of these cases will show that under antiseptic dressings the disease runs very much the ordinary course of a scarlatina infection uncomplicated with any other side issue. The decomposition in the wound being prevented, they do not pass on into the septicæmic condition. The number of cases described in this paper would, of course, be far too small to justify such an inference. But I have taken these cases more

as a text for what I had to say upon the subject than as actually proving my thesis. The inference is really founded upon observations made over a much wider field, viz. that afforded by the Evelina Hospital for Sick Children during the years that I have been connected with it, as well as by the wards of Guy's Hospital. And here I may say that when I was first appointed as surgeon to these hospitals I held the opinion, in common, I suppose, with the majority of surgeons, that a roseoloid rash occurring after operations was only the first sign of septicæmia, and that it afforded no evidence of scarlatinal infection. Gradually, however, I have been forced to abandon this position. The continued observation of these cases has forced upon my mind the conviction which I have recorded above. The frequency with which scarlatina is introduced from without into the wards of a children's hospital has unfortunately afforded only too often the means of observation.

I will conclude this paper by stating the practical lesson which cases such as these teach us, for the sake of enunciating which it has been written.

That for the future any case which develops in our surgical wards a high temperature with roseoloid rash either within a short period after an operation, or during the course of some surgical malady involving suppuration or discharges of any kind, ought to be treated by isolation, just as if it was a proved case of scarlatina, and not permitted to return to the ward until the period of desquamation is quite passed.—Guy's Hospital Reports, vol. xxiv, 1879, p. 443.

7.—ON THE UNITY OF POISON IN SCARLATINA AND PUERPERAL FEVER, TYPHOID, DIPHTHERIA, AND ERYSIPELAS, &c.

By Dr. G. DE GORREQUER GRIFFITH, Senior Physician to the Hospital for Women and Children; Consulting Physician—Accoucheur to St. Saviour's Maternity.

I consider the originating poison of these diseases to be one and the same—in children most frequently causing symptoms we are in the habit of terming scarlet fever; in adults typhoid, and in the lying-in woman puerperal fever. That diphtheria may be originated from the same cause is, I think, clearly demonstrated by the fact that it will be found prevailing concurrently with the other affections, and in the same locality; nay, more, even in the same household, age being apparently one of the strongest determining or predisposing causes, as is shown by Dr. Morell Mackenzie's Tables, wherein out of 1000 cases 540 occurred before the age of five, and 800 before the age

of ten. The same as regards origin holds good of erysipelas; also as to concurrency and attacking persons in the same neighbourhood and house.

This doctrine, identity of poison, differentiation of resulting phenomena—symptoms—is borne out by therapeutical and clinical facts—those facts which set at nought all our calculations and attempts to reduce medicine to an exact science, or to that certainty which would enable us to say positively that the medicines we employ will have no irregular physiological action, but a sure and undeviating effect—the very same we have known them to have had before, and which we wish them again to produce in different people, or the same persons at different times and under different circumstances.

Take, for instance, iodide of potassium. How differently it acts on different patients and at different times of life; and how comparatively innocuous as regards some, especially at the period of childhood?

Take, again, ipecacuanha. How the very whiff of the powder produces, in some, violent sneezing; in others, as violent vomiting and retching, and in others, again, a different train of symptoms. Or, if we name opium. In the majority of persons this drug will produce ease from discomfort, and from pain in certain doses; while in the same quantities it will induce in others excitement, insomnia, griping in the bowels and diarrhoea, as a result of the intestinal irritation. Can anything act more variously than mercury?

I might add an immense number of examples to this catalogue; but these few are sufficient to explain what I mean by therapeutic facts. In the examples I have enumerated—and in many others which could be brought forward—the same drugs in exactly the same doses will be found to act differently in different systems; also differently in the same individuals at different times.

The late Sir JAMES SIMPSON pointed out the analogy between certain forms of puerperal fever and the secondary fever which occurs after great surgical operations, and which, there can be little doubt, is owing to the absorption of purulent matter, or to a *materies morbi* arising from that matter.

Dr. CHURCHILL in his work on the Diseases of Women, says: “We find that *the same seasons give rise to erysipelas, typhus* (under which, in Dublin, typhoid was always included), *scarlatina, and puerperal fever, that they prevail epidemically at the same time; and as an epidemic take on the same type, and appear capable, the one of giving rise to the other, or of co-existing in the same patient.* Further, that the symptoms of certain forms, at least of puerperal fever, are similar to those which occur after great surgical operations, and that the secondary lesions are similar.

“Now, in erysipelas, typhus fever, and the secondary fever after operations, there can be little doubt of the depraved condition of the blood, and it is highly probable that their low typhoid character is owing to this blood-poisoning. I think, therefore, that the conclusion, that the peculiar character and malignancy of certain epidemics of *puerperal fever* also depends upon a morbid condition of the blood, however produced, in addition to the local disease, is inevitable.”

Here is really an enunciation of the views I advocate.

It seems to me that much groping in the dark, much confusion in symptomology and nomenclature, much uncertainty of ideas, much ignorance and consequent perplexity as to the best way for arresting these affections, scarlet fever, diphtheria, typhoid and puerperal, may be avoided by recognising the unity of the originating poison, the power of determination of that poison in individuals (to which I have referred before), so as to cause it to display different symptoms or phenomena leading medical men to consider the above “diseases” to be distinct and different; the powerful influence of the cycle of age, and the amount of the poison taken into the system; by recognising, I repeat, all these factors in determining the development of the symptoms which declare themselves, and have induced medical men to consider these diseases to be the result of separate and distinct specific and morbid matter, when they really are the results of one and the same virus, we shall greatly simplify our study of these affections.

How often an outbreak of scarlet fever, typhoid, erysipelas, diphtheria, or “sore throats,” will occur amongst the children of a family, male and female—among adults—male and female—*after* the mother has shown symptoms of puerperal fever. The outbreak of those ailments in mother, children, and adults of a household at the same time, or almost synchronously, is to be explained by simultaneous exposure to the one poison, whereby all are affected simultaneously—the mother, perhaps, showing the symptoms earlier because of her more ready susceptibility, owing to the changes in the blood and general system peculiar to the puerperal condition; the poison not having been generated autogenetically by the mother first, but being heterogenetic as regards all the sufferers.

Or again, how often will the children of both sexes be attacked with symptoms of the different ailments I have named while the mother is laid low with what is termed puerperal fever, the mother having first been affected; and how often the husband will become also a sufferer just as do the children; and but for the custom of our country, which excludes male visitors, as callers, during the puerperal period, how many more males would be stricken with one or other of these diseases mentioned

—the source of all being the puerperal woman, the origin of her symptoms being the offensive putrescing matter or excretions in utero and vagina, and not contact in any way whatever with any specific infection or contagion.

Nor is there anything contrary to nature in this doctrine of unity of poison, diversity of its results, as evinced by difference of symptoms, the phenomena of its action; for do we not see how from the one source—the blood—the various glands secrete and excrete different and distinct fluids? and how the different tissues of the body—muscles, bones, membranous linings, &c. &c.—are all nourished from the same source?

In the vegetable kingdom do we not find that from the same plot of earth, plants, shrubs, and trees of the deadliest poisonous nature grow, and draw their sustenance, and develop their poison, side by side, and contemporaneously with the most innocuous. So that out of the same soil will spring the wheat and the tare, the flower and the weed, the unpleasant smelling and the most odoriferous, the life-giving and nutritious, as well as the death-dealing.

In this doctrine then of unity, &c., we have no outrage against nature nor the laws by which nature governs the universe.

I would here append some communications which bear most importantly on this subject, and which have been published since I brought the subject before the Profession, through the Paper read at the Obstetrical Society.

Dr. HOLLAND, Assistant Physician to the Hospital for Women, in the British Medical Journal, February 1, 1879, writes:

“Three years ago a suburban school had been broken up from alleged scarlatina; and two of its pupils, supposed to be in health, after a week at home, were received into a large school in St. John’s Wood. All went well for a week, when a French pupil, who slept with the new pupils, failed *with decided scarlatina*, and we found that the two new pupils, though doing ordinary school duties, had a nasal discharge and labial excoriation attributed to cold, and that they were pale, asthenic, and a little feverish. It was also ascertained that they were strumous, and had habitual rhinorrhœa, and that the discharge was only a little increased, and had passed unheeded. During the second week, the pupils fell ill in rapid succession, to the number of twenty-four or twenty-five, and the school was broken up. The invalids presented *every conceivable variety of diphtheria and of scarlatina with and without rash*.

“A was a typical case of scarlatina, with the usual throat and skin manifestations and subsequent desquamation. B was similar, *plus limited false membranes in the pharynx*; C was equally *typical* of recognised *diphtheria*, with false membranes, albuminuria, sequential pallor and paralysis, but with no dis-

coverable rash; and *these varieties presented such a progressive fusion* in their protean combinations that *I regarded the diseased states as modifications of one process, and the evolution of a similar germ in dissimilar pabulum.*

“Dr. A. considered that the cases were scarlatinal; Dr. B., that they were diphtheritic; and Dr. C., that they were only cases of the ‘sore throat that was about.’ It was my lot to inspect all the cases, and to follow the day pupils to their homes; and I know the infection was spread broadcast in St. John’s Wood, and, from the mild phases having been improperly interpreted, many a sorrowing cortège went shrouded to the grave. Two of my children brought the infection home, where eight suffered. The eldest, who had previously had scarlatina, *resisted well*, and were not in bed a day; the youngest two had *malignant diphtheria as bad as could be, short of actual destruction*; they had no rash, but false membranes through the pharynx, nose, and mouth, albuminuria, sequential pallor, paralysis, and quasi-idiocy; and three years of careful treatment has scarcely restored their pristine vigour.

“Here, then, we have an illustration of children only a little unwell, able to do their school duties, with no local disease sufficient to attract the attention of friends, having, in fact, a slight nasal discharge only, and that, unheeded; visiting and kissing in happy innocence, and yet imparting to confiding friends one of the most disastrous, and to be dreaded, of communicable diseases. Surely it cannot require much effort to conceive the possibility and probability of diphtheria being as easily conveyed to a palace, when in disguise, as into the dormitories of a well-appointed school or into the circle of home; especially when children abound and affection abounds, as in the princely home of Darmstadt.”

Dr. BRAXTON HICKS (British Med. Journal, Jan. 4, 1879,) writes:—

“The question raised by Mr. May, jun., of Reading, in the Journal of December 21st, 1878, is a very interesting one, and has exercised the minds of many in late years; and I believe obstetricians generally admit that a rash simulating, or nearly so, that of scarlatina, may be produced by the absorption of unhealthy material; and it is just this which makes it difficult in any given case to say whether the rash be one symptom of scarlatina poisoning, or the effect of the absorption of some deleterious matter. When after the strictest search one has been unable to trace any exposure to scarlatina, the evidence is yet only negative; and in scarlatina proper how many cases daily occur where no evidence of contagion can be found!

“But the following case is more interesting and conclusive

even than the one quoted by Mr. May (because it might be said in his that there had been inoculation), and is curious as having occurred in the same town. My son was at school at Reading in 1873, and was seized with a very severe attack of pyrexia, and on the second day with delirium, with a rash all over him, very like that of scarlatina. There was no particular redness about the throat, though it was somewhat blushed. As he was delirious, we could not gain much information from him for the first three days; but as this passed off, he complained very much of the right buttock, near the tuberosity of the ischium, which region was fuller than the other. Suffice it to say, that this proved the commencement of a very large deep-seated abscess, which had to be opened. Inquiry as to the cause showed that while he was standing a few days before with his legs wide apart, a school-fellow had still further stretched them out by pulling at one. The rash lasted about three days; was nearly universal from the first; but was not followed by any desquamation of the cuticle. But this symptom of exfoliation is nothing characteristic of scarlatina; any other inflammation to an equal extent, of the same parts, will produce it.

“Here we have a simple strain of the muscles and fasciæ, producing, without the smallest abrasion or wound, a rash having so much resemblance to that of scarlatina that at first it was doubtful how far it was scarlatinal. Here was no question of inoculation.

“That highly irritating material can be effused into the cellular tissue, as the result of traumatism, I had once abundant evidence in myself.

“These causes can be viewed another way; namely, that a wound or damage existing, the presence of the exanthem in the system so disturbs it, that instead of natural recovery ensuing, the part runs on to suppuration.

“In the case of my son, there had been at the school no case of scarlatina for a long time, nor was any one affected who afterwards came into contact with him.

“But in the case of wounds followed by a rash, one cannot avoid enlarging our inquiries as to the capability of scarlatina being transmitted by inoculation; and also, if it can, what are its features when so received.

“I trust observations will be continued on this subject.”

Dr. CLEMENT DUKE (British Med. Journal, Jan. 25, 1879), writes in reference to the relations between diphtheria and typhoid fever:—

“That the same poison which originates typhoid fever, often causes diphtheria; that the two diseases often occur side by side in different individuals, in the same house; and that therefore we must look for their same source of origin, the following cases show.

“1st. On October 30th, 1875, I was called in consultation some distance in the country, to a large private school at Y—to see Mr. R—, who was supposed to be suffering from typhoid fever, though he was not. There were, however, five or six cases of typhoid fever in other rooms that I saw. While these cases were in progress, and within a few days of my visit, a younger child developed pharyngeal diphtheria, and died. These cases occurred at a large house, standing by itself in several acres of ground; they were traced to the well water (there was no other water supply); there had been a great rainfall, the cesspools had filled and overflowed, and the well being near and on a lower level than the cesspools, the overflow had found its way into the well, which was marked by making the side of the well by which it entered, slimy and blackish.

“2nd. At the beginning of January, 1875, Mr. T—, of Rugby, developed diphtheria, which was followed by paralysis. In the same house, on January 15th, 1875, Mr. T—’s servant sickened with typhoid fever, went through a severe illness, but recovered.

“During the last days of December, 1874, there being something amiss with the pipes which carried the water supply from the town to the cistern, the water was cut off for one day to remedy the defect. On that day, a pump in a yard at the back of the house was used for their water supply; this well was much fouled from adjoining stables. There was, however, another possible source of poison; for the trap to the water closet was defective, and allowed an escape of sewer gas into the house. Both sources of poison were remedied, all the family were sent out of the house, and we had no further cases.”

Dr. G. W. THOMSON (British Med. Journal, Feb. 8, 1879,) writes in reference to the same subject:—

“As a sequel to Dr. Duke’s Paper in the Journal of January 25th, and as showing in some degree that the poison from which diphtheria may arise is similar to that of typhoid fever, the following remarks are made:—

“At the time of the outbreak, which I am going to narrate, I had not had a single case for some time previous, of either diphtheria or typhoid fever. Suddenly, however, within a week, ten cases of mild typhoid fever and six cases of what I looked upon as mild diphtheria (yet having all the characteristics of what has recently been designated spreading quinsey) occurred. Such naturally led me to look about for a cause, and a very short inspection sufficed.

“This outbreak occurred in a small mining village, most of the cases occurring in those rows which were built on low-lying ground, or were closely confined; while those through which

the wind penetrated, though as filthy as the others, escaped. Previously to the outbreak, the ground for four or five weeks had lain covered with snow to the depth of from four to eight inches. During this period every kind of sewage seems to have been thrown from the doorsteps of the houses instead of carrying it to the ash-pit, the snow for the time hiding everything from sight. A sudden and complete thaw setting in revealed a not very creditable state of matters; filth of every variety lying around the doors, the snow having proved for the time but a whitened sepulchre. It was immediately after this exposure that this small yet distinct outbreak set in.

“There was nothing particular about the attack; things were cleared up, more snow fell, the cases were all mild, no deaths occurring.

“Although I have mentioned six as the number of throat cases (all of which had the distinct patch on the tonsils and the accompanying inflammation and debility), many other throat cases at the same time were common, but so mild as not to require medical aid. I have also from this number been careful to exclude four sharp attacks in different individuals of acute tonsillitis. Whether these six cases were cases of pure diphtheria, or of ‘spreading quinsey,’ I do not venture to decide. It was evident, however, that it was a filth disease, arising from an organic impurity in the air. This impurity was caused by the churning action (which the rapid disappearance of the snow occasioned) on the filth which lay around the windows and doors.

“I may say that *in one house* there were two patients *laid up at the same time*, one with the ordinary symptoms of mild *typhoid fever*, running its course in three weeks, while *the other* had mild *diphtheritic sore throat*, recovering in about eight days.”

Dr. J. A. LEA (British Med. Journal, Feb., 1879) makes the following remarks on “Scarlatina and Surgery”:—

“The following case seems to show the existence of a non-contagious scarlatinal rash:—

“A child, aged five, suffered from inflammation of the lymphatic glands in the groin, which suppurated. At the time when the abscess was fully formed, a rash appeared all over the body. This rash was paler than that of scarlet fever, and consisted of a number of red points with pale spaces between them: not the patches of scarlet fever. The temperature never rose above 99 deg. She did not suffer from sore throat, and there was no perceptible desquamation of cuticle. Although this child was among a number of other children, no other person became affected, nor could her illness be traced to any source of infection. Is it possible that the phenomena of scarlet fever are produced by more than one kind of irritant? and that the

irritant causing contagious scarlet fever differs from the other in being reproducible in the body?

“Such would seem to be the case in diphtheria and yellow fever; as there are cases of diphtheria which seem to be non-contagious, and cases of fever which closely simulate yellow fever, but differ in not being contagious.

“The following case of scarlet fever happening in a surgical case exemplifies the remark of the late Mr. Solly, quoted by Dr. Braxton Hicks. ‘Whenever a case of surgery in private takes on a highly phlegmonous appearance, I am always sure to find break out in the inmates of the house *either erysipelas or scarlet fever.*’ It exemplifies also the remark of Dr. Hicks about *the variations of symptoms in an exanthem.*

“J. H., aged thirty-nine, suffering from acute necrosis of the tibia, became worse twenty-four hours after an abscess was opened. His temperature, which before opening the abscess, was never higher than 101 deg., went up to 104·8 deg., his tongue became red and glazed, and *the wound put on a phlegmonous appearance*, the discharge being scanty and thin. He had neither rash nor sore throat. In a short time his urine became albuminous, containing renal casts and blood corpuscles. Desquamation of the cuticle followed.

“Some days before the abscess was opened, one of his children was attacked with scarlet fever contracted at school. All the other children in the house were successively attacked.

“It is worthy of remark, as being some evidence in favour of the belief that surgical patients are more susceptible to a contagious poison, that J. H. was the only adult who was attacked during the epidemic in which a number of children suffered.”

Dr. GRIMSHAW, Physician to the Dublin Fever Hospital (Obstetrical Journal, Feb., 1879), is stated to have said:—

“I was called on to attend three or four cases of women after confinement, all of whom *I* believe were suffering from *enteric fever*. In two of these it certainly was so. In one case I was able to trace the disease as having been contracted in a court where she was living, having come to town with the intention of going to the Coombe Lying-in Hospital to be confined. She showed no serious symptoms of fever until *after her confinement*, when *symptoms of puerperal fever* were set up, with morning and evening variations of temperature. She died, and a *post-mortem examination disclosed the characteristic lesions of enteric fever.*

“That case conveys the very important lesson that there may be a considerable number of cases *apparently of puerperal fever, but which are not really of that nature.* I do not mean to say that all of them, or the greater portion of them, are so; but it

certainly is a remarkable fact that amongst the cases I saw at the Coombe, *some were scarlatina, some were this very enteric fever, and some turned out to be nothing at all.* Four of them were sufficiently well-marked cases of enteric fever to be recognisable; and in one the *post-mortem* confirmed the diagnosis."

These observations of Dr. Grimshaw forcibly recall to my mind the words I spoke at the Obstetrical Society in 1875 (Obstetrical Trans., vol. xvii., p. 264) on the subject of "Puerperal Fever and its Relation to the Infective Diseases and Pyæmia:—

"Bearing in mind, therefore, the life symptoms which exist in scarlet fever, puerperal fever, and typhoid; also the pathological conditions found after death—and how closely allied, how much alike they are—both the life and after-death phenomena—we can see how readily scarlet fever can run into puerperal—puerperal into typhoid (and *vice versa* as regards them all); how typhoid came to be supposed to originate puerperal fever [Sir Henry Marsh's opinion]; and how puerperal fever may so clothe itself in the garments (symptoms or phenomena) of scarlet fever that there are really no distinctive marks."

From the various facts which are adduced in this paper, and from the evidences which I have accumulated since writing it, I consider we are warranted in holding that in those diseases there is a unity of poison, a common sepsis. Nor is this in anywise contradicted by the fact that puerperal fever may result from putrefying and offensive discharges, and even from healthy or apparently healthy lochia, flowing over wounded surfaces in the uterus, vagina, or pudenda; for the sepsis having in this way entered the blood, sets up in it, and in the tissues of the body generally, changes identical with those resulting from the infection or contagion which induces what in contradistinction we may term the non-traumatic form of puerperal fever; just as any other fevers—scarlet, typhoid, &c., &c.—are set up; the traumatic or autogenetic form of puerperal fever being as deadly as the non-traumatic or heterogenetic, the sepsis or virus being one and the same, no matter how it be introduced in the system, the symptoms resulting or the phenomena developed subsequently and as a direct consequence being the same or nearly so in a number of cases, or altogether different as the case may be, dependent upon the factors of the difference which I have before pointed out.

"It is doubtful," says MATTHEWS DUNCAN, in his Paper on "Antiseptic Midwifery," "whether or not danger ever arises spontaneously within.

"The germs or septic matter, which set agoing ordinary surgical or puerperal septicæmia and pyæmia, are certainly in

the majority of cases, if not in all, imported into the female system from without.

“But when they are introduced, the evil is seated within. *Obstetrical septicæmia* from putrefaction is seen when the putrefaction occurs, and as long as it proceeds. *Ordinary septicæmia*, on the other hand, is, as is well known, the product in ordinary circumstances of the first few days after delivery, while wounds are still fresh and absorbing. The danger of the *septicæmia by putrefaction* is a uterine danger; for it is almost exclusively in the uterus that dangerous putrefaction occurs. The danger of *ordinary septicæmia* is extensively believed to be dependent on lacerations.”

But the same virus which originates scarlet fever, typhoid and diphtheria, will induce symptoms identical with those resulting from the two kinds of lying-in septicæmia, described by Dr. Duncan—any difference being due to the causes, and for the reasons already recapitulated in this paper—the sepsis or virus being the same—no matter how introduced—auto- or hetero-genetically—the phenomena or symptoms being different in different patients.

If there be no other advantage in holding these views, there is certainly this—that it simplifies the study and treatment of these *seemingly* different affections.

Since writing the foregoing, which I sent to Dr. John Harley on its completion, and which he perused, I have received from him the following:—

“I think you have made out a very good case. My own conviction has long been that febricula, enteric fever, scarlatina, diphtheria, quinsey, puerperal fever, septicæmia, et hoc genus omne, are natural links of one connected chain, which the love of *unnatural classification* has dissociated.

“I rejoice that the clear light of truth is beginning to dawn on the subject; and I think your efforts will much conduce to a clear understanding of this natural association, relationship, and connection.”

Surgeon-Major FFOLLIOTT publishes a case in the British Medical Journal, April 5, 1879, which he terms one of “Septicæmia simulating Scarlet Fever,” but which will, on reading in the light of the views held out in this Paper, be seen to be a well-marked though mild instance of what I have named elsewhere “toxæmic scarlatina,” resulting from the irritating fluid generated in, and from, wounds created by a severe burn, that irritating fluid with which every operating surgeon is familiar, as being most dangerous to the welfare of the patient who has undergone an operation. I will here give Dr. Ffolliott’s report *in extenso*, and let it speak for itself:—

“Private Chapman, coming in the vicinity of the powder

magazine, threw a lighted match on the ground after having lit his pipe. This ignited a quantity of powder that lay scattered on the ground, which he had not observed; the result was, that he was, as he put it, 'blown up,' being severely burned on the left hip and inside of the thigh, and on the arms and face. The accident occurred on December 21, 1878, and the following day he was sent to Peshawur, to the Base Hospital, for treatment. On the third day after his admission (Dec. 25) there was considerable constitutional disturbance; *a bright scarlet rash* was found on the abdomen, *which spread over the whole body*. The following day the *eruption was very vivid, resembling a boiled lobster*. Several medical officers who saw the case, pronounced it *scarlatina*. Precautionary measures were taken, and the case was isolated. The eruption continued for five days, and then declined gradually. After the eruption appeared, the febrile symptoms subsided, the temperature becoming normal. The highest temperature recorded was at the onset, when the thermometer registered 101°. The tongue was lightly furred, never at any time becoming red, nor was there any soreness of throat or enlargement of tonsils. The cuticle desquamated over the whole body, as in true scarlatina, even the palms of the hands and soles of the feet not escaping.

"Although scarlatina is not unknown in India, I may state that, after the service of twelve years in the majority of the military stations of the Punjab, I have not seen or heard of a single case of this disease among Europeans or natives. There was no such disease in camp as far as can be ascertained.

"The soldier was twenty-four years of age, and has been three years in India. He recovered from the burns in due course, without any further ill effects, and was discharged from hospital, on February 3, 1879.

"There can be very little doubt that the absorption of the septic poison into the circulation is capable of producing a scarlet rash on the body; whether this is capable of transmission by infection, I am unable to state. I doubt it very much, and think, when such is the case, the disease will be found to be true scarlatina."

Mr. ERICHSEN, President of the Medico-Chirurgical Society, in the discussion on the report of the Committee on Croup and Diphtheria (British Med. Journal, April 19, 1879) states:—"Diphtheria is by the report said to be a *zymotic* disease, accompanied by a membrane which might or might not be attended with croup." Croup being defined to be "a laryngeal obstruction associated with pyrexia in children, which might be accompanied or not with false membrane." In the discussion which followed, Mr. R. W. Parker speaks of "*zymotic* sore throat;" and also says "true croup prevailed

in common with other zymotic diseases, or independently of them." "In accepting a zymotic cause for the origin of all croupous diseases, he did not know one single fact which militated against it, whilst he recognised many which were greatly in its favour; and if in the future it should come to be regarded that a zymotic cause was at the bottom of the disease, then objections to the identity of croup and diphtheria would lose all their force." Here is the evidence of a medical man who would place croup and diphtheria (they being almost universally by the profession supposed to be quite distinct ailments) amongst the zymotics, who speaks of croup "*prevailing in common with other zymotic diseases*," and of "*accepting for the origin of all croupous diseases a zymotic cause*," and thus bear me out in the doctrine advocated, unity of poison, differentiation of resulting phenomena (symptoms) for the reasons I have already mentioned.

Dr. WILSON FOX remarked in the course of the debate: "It was exceedingly difficult to say there had been no zymotic cause in all cases; and he believed that diphtheria might arise *de novo* . . . the non-presence of any zymotic condition was always very difficult to prove."

Mr. HOLMES said: "The term diphtheria is the anatomical definition of a zymotic disease which may, or may not, be attended with croup."

Dr. MCINTYRE: "Diphtheria is a blood disease, with local mischief."

Dr. GEORGE JOHNSON (same debate continued, and reported in British Medical Journal, May 3, 1879) says: "A remarkable feature of the diphtheritic poison is its tendency to attack inflamed or abraded surfaces" (in this, I would here point out, resembling erysipelas). "Trousseau had described cases of infection of abraded cutaneous surfaces by the diphtheritic poison; and in scarlet fever, measles, typhoid, &c., &c., there was often a diphtheritic exudation in the throat—'this being,' he adds, 'a distinct disease.' I should say it was merely another manifestation of the same blood poison. A case had been related to him (Dr. Johnson) by Dr. Dewes, in which two boys had been sent from (observe) *the one* home, one suffering from scarlet fever, the other from scarlet fever with diphtheria. When they were convalescent, and came together, the first was attacked with croupy symptoms and died of diphtheria."

Mr. JONATHAN HUTCHINSON said: With regard to the connection between diphtheria (of the conjunctiva) and scarlet fever: this latter disease was sometimes accompanied by diphtheritic ophthalmia, and the conjunctiva became covered with a membrane, the removal of which readily produced bleeding. He had seen several cases in patients recovering from scarlet fever." "Some

predisposing force" (the unity of poison, I would say) "in scarlet fever, led to the formation of a pellicle, *apart altogether from any special exposure to diphtheria*, and in this, as in other respects, the facts as regards membranous conjunctivitis were remarkably parallel with those of membranous pharyngitis. In another class of cases the diphtheritic pellicle attacked wounds. He had met with a case in which a boy, who had a compound fracture of the leg, was thus affected; he had had an attack of diphtheria nine months before, and at the time of the accident was carried into the house where diphtheria was present. The pellicular condition of the wound was very marked in this case. He had also seen pellicular deposits on wounds in many cases, chiefly in connection with hospital gangrene, which occurred in the London Hospital ten years ago. These facts were of interest with reference to the opinion of Virchow—in which he, Mr. Hutchinson, thought there was much truth—that *diphtheria should be placed in the same category as hospital gangrene.*"

In the debate on puerperal fever, held at the Obstetrical Society in 1875, Mr. Spencer Wells speaks of "diphtheritic exudation on the mucous membrane of the uterus and vagina, especially on the place of the separated placenta, *occurring during puerperal fever*;" and asks, "Does puerperal fever always and necessarily depend on the action of a morbid poison?" These words I would like to place side by side with those of Mr. J. Hutchinson, as bearing out the doctrine advocated in my Papers.

"Then, as to membranous angina, he (Mr. Hutchinson) would ask whether a contagious sore throat of frequent occurrence as an epidemic, but not fatal, in which there were a few patches of deposit, but no large skins, was diphtheria, or a separate disease? In a part of the country with which he was well acquainted, this sore throat spread from house to house, without being traceable to brain affection, or any other apparent cause. No one died of it; though many were very ill. The disease was attended with inflammatory fever, and with the formation of small patches on the throat, but no large pellicles. The epidemic lasted several months, and was very contagious. It began in a village with a condition which was called diphtheria, three or four cases of which were fatal." Here assuredly are samples of unity of poison, differentiation of symptoms, &c., &c.!

"He (Mr. J. Hutchinson) could make but little distinction between these sore throats and, what was called, hospital sore throat; and he would ask the Committee whether they recognised *hospital sore throat* and *common contagious pharyngitis* as *minimised forms of diphtheria*? If they were so, what of the doctrine of the specific nature of the latter? The definition

of diphtheria as a *specific fever is wrong*. In many cases (wherein what was termed diphtheritic paralysis had occurred) he had found, after close inquiry, that there had been only slight sore throat. One case was that of a gentleman who had *only a temporary slight sore throat*, while nursing a *child who was ill with diphtheria*." Proof of the truth of unity of poison; differentiation of symptoms!

"His (Mr. Hutchinson's) object *was to question whether there was any sound reason for speaking of diphtheria as a specific fever*... its onset was very like that of erysipelas."

"The adoption of the view, that *diphtheria was not a specific fever*, but rather a specialised form of local inflammation, would remove much difficulty." A specialised form of local inflammation dependent upon a poison at one with that setting up what are termed specific diseases, some of which I have placed at the head of this Paper.

Dr. SQUIRE said: "The only part of the Report to which he would take exception, was that on the anomalous character of diphtheria as an acute specific disease. *It was likened to typhoid fever; but he considered it had a close analogy to scarlet fever*, and that all the precautions necessary against one disease should be used against the other. In this respect—that diphtheria always increases as an epidemic in the early winter—diphtheria agrees with enteric and scarlet fever."

Dr. FITZPATRICK and Dr. JOHNSON consider diphtheria may be originated *de novo* from sewage contamination. Dr. F. Thorne's words bear out so much of the view of this Paper that I will quote them from the British Medical Journal, May 24, 1879:—

"As long as the exudation was limited to the larynx, and there was no evidence of any previous cases, the disease was called croup; but when the disease spread, and the subsequent cases were plainly derived from the early ones, it was called diphtheria. With regard to the etiology of the disease, he had made several investigations under specially favourable circumstances in isolated districts in which the *earliest attacks were certainly not contracted from contact with persons having diphtheria*.

"Over a large area, too extensive for personal infection, *there would be a great tendency to sore throat*. Then, groups of cases would occur in which the disease was *decidedly infectious*; and, later on, there would be severe cases with patches of membrane on the tonsils, and inflammatory symptoms; or with obscure inflammatory symptoms, difficulty of swallowing, and loss of voice—but no visible exudation. At last, perhaps in a single village in the area, there would be an *outbreak of diphtheria in its severe and fatal form*. Hence the diphtheria appeared to be developed from what was originally a simple sore throat, and the infectious character was gradually increased."

Can any words more fully confirm the view of unity of poison, differentiation of resultant phenomena—symptoms—and of the views, dependent upon this unity, &c., as the corner-stone?

I would here quote words from the “leader” of the Medical Press, May 28, 1879, significantly confirmatory of the views of the unity of poison, differentiation, &c., &c.:—

“It had been observed that in several outbreaks of diphtheria, the earliest deaths were referred to membranous croup, and the later ones to diphtheria; and that, taking observations extending over a large area, it seemed as if diphtheria was developed from what was originally a simple sore throat, the infectious and diphtheritic character of the affection gradually increasing, till in some districts there would be an outbreak of diphtheria in its severe and fatal form. Might not, therefore, so-called membranous croup occupy an intermediate position in the development of the disease? It is *very natural* that the first cases of an epidemic outbreak of diphtheria should be of a mild form, and that these cases, wanting many of the most characteristic features of the more severe and fatal form of diphtheria, should be set down as croup—just as *epidemics of cholera are preceded by an unusual prevalence of ordinary diarrhœa; so may epidemics of diphtheria be ushered in by several forms of throat affection varying from common sore throat to attacks from what can scarcely be distinguished from inflammatory croup.*”

Dr. DICKINSON says: “There might be a membranous affection of the larynx in small-pox or scarlet fever, but this was very rare; and one could not say that diphtheria might not also have been present in such cases.” Is it not a law in nature that two things cannot occupy the same place at the same time? Is there any proof that there are two distinct specific *acute* poisons in the blood at the same time, producing—one scarlatina, as is the fashion to call certain symptoms, another diphtheria—as *though they were distinct entities*?

Is it not much more in accordance with all scientific knowledge and with facts, that, instead of there being two separate *specific* acute poisons in the blood, producing two distinct diseases, there is one virus, producing diseased actions—demonstrating its presence by different symptoms or phenomena, for the reasons I have already pointed out?

Dr. Dickinson continued: “There was evidence that *diphtheria was interchangeable with follicular tonsillitis*, and he thought that it had also *distinct pathological connection with other diseases*, and called to mind a series of cases in which *poisoned water*—out of one well into which ran drainage—had been drunk, the result being, *in various cases*, diarrhœa, purulent ophthalmia, erysipelas, pharyngeal diphtheria, and croup or laryngeal diphtheria—all arising from the same cause. There is curious evidence that

diphtheria may be *only one of several disorders engendered by one and the same cause!*" How strongly affirmatory of the doctrine, unity of poison, differentiation of symptoms, &c. &c.!

"So far as Buchanan's figures go, they are in favour of the view that certain conditions, apart from special diphtheritic influence, may develop membrane in the air-passages. The certain conditions are, "the various exanthems." "The probability is," pursued Dr. Dickinson, "that these febrile and accidental irritants are able themselves to produce the membrane in question." I have explained that the unity of poison, differentiation, &c., will account for the occurrence of these so-called different affections; and for their *concurrence* in the *same* persons, and their *occurrence* in *different* persons *at the same time*—*Obstetrical Journal*, June and July, 1879, pp. 134, 222.

8.—ON THE TREATMENT OF ACUTE RHEUMATISM BY SALICIN AND SALICYLIC ACID.

By Dr. T. J. MACLAGAN, Examiner in Medicine to the University of Aberdeen.

Two questions are frequently put to me: First—Are salicin and salicylic acid antipyretic? and, if so, is their beneficial action in acute rheumatism due to their antipyretic effect? Secondly—Which is the better preparation, salicin or salicylic acid? The questions are important. I shall answer each in detail.

First, as to their antipyretic action. Fever is a collection of different phenomena whose coexistence constitutes the febrile state. Of these phenomena the most essential and most prominent is rise of temperature. Hence fever has been defined as *calor præter naturam*; and one whose temperature is above the normal is said to be feverish. A remedy which reduces or removes this abnormal rise is said to have an antipyretic action. Such a remedy may act in one of two ways. Either it may remove the condition—i.e., cure the disease—to which the rise of temperature is due; or it may reduce the temperature of the body without curing, or even curtailing the duration of, the malady. An instance of the former we have in the treatment of intermittent fever by large doses of quinine. An instance of the latter we have in the action of the same drug in some other forms of fever. A further and more striking example we have in the external application of cold to the febrile body. Though salicin and salicylic acid do sometimes have an antipyretic action, their effect in this way is not marked, and is not to be compared to that of quinine. I have given both salicin and salicylic acid frequently and freely (thirty

grains every hour for six hours, and every two hours for three or four days) in typhus, typhoid, cerebro-spinal fever, scarlet fever, diphtheria, and pneumonia, and in no case was there ever produced any decided antipyretic effect. On several occasions I have seen the temperature pulled down (temporarily of course) two or three degrees by a couple of ten-grain doses of quinine, after frequently repeated thirty-grain doses of salicin and salicylate of soda had failed to have any influence on it. My answer to the first question, then, is—No; salicin and salicylic acid are not antipyretic to any useful extent. They are anti-rheumatic; and their beneficial action in acute rheumatism is due, not to their allaying the fever, but to their putting a stop to the whole process of the disease, and to all that constitutes it—the fever as well as the other symptoms. As a rule, relief of pain precedes fall of temperature.

Second, Which is the better remedy of the two? It was in November, 1874, that I began to use salicin. When, a little later, salicylic acid was introduced as an antiseptic, and before anything had been written of its antipyretic action, I tried it too as a remedy in rheumatism. It benefited the rheumatism, but caused, at the same time, so much irritation of the throat and stomach that I abandoned it in favour of salicin, and did not try it again till after the publication of Stricker's observations. For the last three years I have used the two remedies in about equal proportions. The result has been to convince me that salicin is the better remedy of the two. As this is not the generally accepted view, it may be well to indicate, first, the reasons why salicylic acid is more used than salicin; and, second, my reasons for regarding this preference as misplaced.

Salicin is prepared from the bark of different species of willow. The bark is removed in spring, when it contains the largest quantity of the bitter principle, so that the quantity in the market during the summer represents all that is to be had till the following spring. Previous to the publication of my paper, salicin was scarcely ever prescribed, and was kept by chemists chiefly as a curiosity. There was very little of it in the market. At that time I resided in Dundee. Before publishing my paper, I asked the leading chemists there to lay in a good stock of the drug, as I anticipated there would be a considerable run upon it. They did so, and I thus had the advantage of having at my disposal for further observation a good supply of the pure drug. The anticipated result took place. There immediately sprang up a great demand for it. The price when my paper was written was two shillings an ounce. It speedily rose to six, eight, and even twelve shillings; and ultimately ceased

for a time to be quoted in the druggists' monthly lists. The demand far exceeded the supply, and no more bark could be had till the following year. And yet chemists continued to prescribe it. They could not have prescribed pure salicin, for it was not to be had. The combination of rise in price, great demand, and insufficient supply, led to the usual result of such a combination—adulteration. The substance used for this purpose was boracic acid, and much of what was sold as salicin was, I have been informed, a mixture of boracic acid and salicin, or even of boracic acid and quinine. For this English chemists were not to blame. Salicin was made at that time only in Germany, and was probably prescribed here by retail chemists just as it was imported. To this adulteration of the drug is probably due the unsatisfactory results which some physicians got from it at the time to which I refer. It is now manufactured largely in this country as well as in Germany, is back to the old price, and there is not the same temptation to adulterate it. Those who were formerly disappointed I would ask to try it again. The high price of salicin and the difficulty of getting it led to the free use of salicylic acid, which could be got cheaply and in any quantity by the new mode of preparing it from carbolic acid.

As anti-rheumatics the two agencies are on a par; acute rheumatism seeming to be as effectually and as speedily cured by the one as by the other. Equally good effects being got from both, it necessarily followed that the cheaper and more easily procured remedy got the preference over the dearer and scarcer one. Thus salicylic acid came into more general use than salicin. But there was yet another reason for this. Immediately after the publication of my original paper, Senator drew attention to it in an article in the *Centralblatt*, in which, as well as in a subsequent and more elaborate communication in the *Berlin Klin. Wochenschrift*, he expressed his preference for salicin, as being more efficacious than salicylic acid. He further gave it as his opinion that salicin is converted into salicylic acid in the blood; and that its greater efficacy is due to the fact that it thus exercises its remedial action while in the nascent state. This idea of Senator's, that salicin is converted into salicylic acid in the blood, and that salicylic acid is therefore the true remedial agency, has been accepted by the profession with a readiness which, considering the absence of evidence to support it, is to me surprising. It is a mere hypothesis, in support of which Senator has brought forward nothing worthy of the name of evidence. The fact that a blue colour is got when perchloride of iron is added to the urine indicates, not that salicylic acid has been taken or formed, but merely that one of the salicyl compounds exists in the urine;

salicyluric acid, salicylic acid, salicylous acid, saligenine, would all give the same coloration. The fact remains, however, that Senator's idea was accepted; and that it was, and is, generally believed that salicin owes its anti-rheumatic virtues to its being converted into salicylic acid in the blood. His further idea, that the nascent salicylic acid thus formed is more potent than that taken by the mouth, does not seem to have been so readily grasped or understood. If, it has been argued, salicylic acid be the true remedial agency, why not give it at once and directly, instead of in a roundabout way? The result of this mode of reasoning has been a preference for, and the more general employment of, salicylic acid. It is curious to find that Senator himself prefers salicin, while those who pretend to follow him prefer salicylic acid; and that his reason for preferring the former is regarded by them as a reason for preferring the latter.

As already remarked, Senator's idea is a mere hypothesis. It is quite possible that salicin may be converted into salicylic acid in the blood; but it is not impossible that salicylic acid may be converted into salicin; and more likely than either is it that both are converted into some other third substance. But there is no need for any such hypothesis. It is quite within the bounds of probability that two allied substances, such as salicin and salicylic acid, should exercise an equally beneficial action in a given malady; and our recognition of this remedial action does not impose upon us the necessity of denying the separate and independent action of either. The fact is that we know nothing certainly either of the changes which salicin and salicylic acid undergo in the system, or of the manner in which their anti-rheumatic effect is produced.

But I would do more than deny the existence of evidence in favour of Senator's view; I would assert the existence of positive evidence against it. For if that view were correct, if it were the case that salicin owed its therapeutic effects to its being converted into salicylic acid in the system, then ought both remedies to have the same action on the system. Now, though their action on the rheumatic poison is the same, their action on the system is not so, as is evidenced by the different results which are frequently got from their separate administration.

1. It is a fact that salicylic acid and salicylate of soda not unfrequently give rise to considerable and even alarming depression. Such an untoward effect is not produced by salicin. From a therapeutic point of view this is one of the most important points of difference between the two remedies. In a disease, such as acute rheumatism, in which the heart is apt to be involved, the absence of this tendency to cause depression

points out salicin as a much safer remedy than salicylic acid. Its superiority in this respect is specially referred to by Senator, who, curiously, does not seem to see that the fact to which he directs attention is a strong argument against his view that salicin owes its therapeutic virtues to its being converted into salicylic acid in the system.

Of the depressing action of salicylic acid many instances are recorded. Several have come under my own notice. The following is of value as the unbiassed evidence of an intelligent, well-informed medical man, founded on his own experience of the two drugs. My friend and then neighbour, Dr. Sinclair, of Dundee, now physician to the infirmary of that town, suffered from an attack of subacute rheumatism last December. Before I saw him he had been taking salicylate of soda in twenty-grain doses with relief to the pain; but it so depressed him, and made him feel so wretched, that he said he could not go on with it. I recommended salicin instead. He took it in even larger doses than the salicylate, with speedy relief to his rheumatism and without any untoward effect. On the contrary, he seemed, under its influence, to regain strength and appetite, and was soon quite well. The following is his own statement, given with his permission:—"Both drugs relieved the pain, tenderness, and swelling, when taken in full doses frequently repeated. But the salicylate, which I employed first, produced some very unpleasant effects. The taste I found to be disagreeably sweet and nauseous. After taking several twenty-grain doses, a copious perspiration was produced; the strength of the pulse was very distinctly diminished, while its frequency was increased; and a feeling of most uncomfortable depression, with singing in the ears, ensued. Indeed, I hardly knew whether the disease or the remedy was preferable. Salicin, on the other hand, has a pleasantly bitter taste; it improved the tone of my pulse and digestion, and relieved the pains more rapidly. Neither drug gave any relief except when taken in twenty or thirty-grain doses every hour for from six to twelve consecutive hours. It may be said that, had I taken smaller or less frequently repeated doses of the salicylate, I might have escaped all the disagreeable effects except the taste—itself no small matter. But such doses produced no effect on my rheumatism. To my mind one of the great merits of salicin is the absolute safety with which large doses can be taken. In the course of one period of twenty-four hours I swallowed an ounce of it with nothing but benefit."

I have seen salicylate of soda produce very alarming depression, closely resembling that of the typhoid state. Not long ago I saw in consultation a case in which it was a question whether the fatal result was not due to the depressing action

of the salicylate. By some this effect has been attributed to the presence of carbolic acid, consequent on faulty preparation. Such an explanation may have been applicable to some cases, but is not so to all. I have more than once seen marked depression produced by a solution of salicylate of soda in which no trace of such impurity could be found, and which was given to another patient in the same dose without causing any unpleasant effect. The worst effects that I have ever seen follow the administration of large doses of salicin are a sense of fullness in the head and singing in the ears; such symptoms are commonly produced by large doses of quinine.

2. Further evidence against Senator's view of the mode of action of salicin we have in the fact that salicin cures cases of chronic rheumatism and of neuralgia in which salicylic acid fails to produce any effect on the ailment. Two instances I shall give by way of illustration.

Mrs. R—, aged thirty, the mother of four children, had rheumatic fever when she was sixteen, and again when twenty-two, shortly after the birth of her eldest child. Since then she has been subject to chronic pains, which are worse in damp weather, and affect chiefly the back and thighs. When seen in May, 1878, she complained chiefly of the thighs, the rheumatic affection seeming to have its seat in the fascia. She moved about the house with some pain and stiffness, and was unable to go out. The temperature was normal. There was some prolongation of the first sound at apex. I gave her twenty grains of salicin every two hours. The next day the pains were much less, and on the following day she felt quite well. She took twenty grains of salicin three times a day for ten days, and at the end of that time expressed herself as feeling better than she had done for years. In November of the same year I saw her again, suffering in the same way. This time I gave salicylate of soda in the same dose as I had formerly given salicin, twenty grains every two hours. On the following day she was no better, but complained of feeling weak and giddy. She begged me to give her the powders again. I gave her twenty grains of salicin every two hours, and on the following day found her much better, the pains nearly gone, and the giddiness entirely so. She was quite well in two days.

A lady consulted me regarding a periodic neuralgia affecting the left supra-orbital nerve. The pain came in the evening. She had taken many remedies. Quinine removed the neuralgia, but gave her such intense headache, and made her so ill for days, that she dreaded its effects quite as much as the neuralgic pain. I gave her thirty grains of salicin every two hours. On the evening of the day on which she began to take it the pain

returned as usual, but she thought it less severe, and it lasted for a shorter time. She went on with the salicin, and the next evening there was no pain. She remained well, but continued the salicin every four hours for some days. Three months later the pain returned in the same nerve, and had the same periodic character. This time I gave salicylate of soda in the same dose, thirty grains every two hours. It made her head feel very heavy, and herself very uncomfortable, but did no good to the pain, though she continued it for two days. At the end of that time it was omitted, and salicin given in the same dose, and with the same result, as before. The pain vanished, and did not return.

In the face of the evidence which has been given, it seems to me impossible for us to accept Senator's view that salicin is converted into salicylic acid in the system, and that it owes its therapeutic virtues to such conversion.

Salicin and salicylic acid are two distinct substances. Being so, they not unlikely have different actions on the system. It is possible that they may be eliminated from the system in the same form. There is some evidence to show that such is the case, and that both are eliminated as salicyluric acid. But it is to be specially noted that their therapeutic effects have been produced, and their full action on the system exercised, before they have reached the stage of elimination and before they have undergone the changes which immediately precede it. Observation and evidence show that their action on the system is different—that the action of salicin is tonic, while that of salicylic acid is depressing, sometimes alarmingly so. This difference, be it noted (and the point is an important one), is quite compatible with their exercising an identical action on the rheumatic poison, and evidence all tends to show that their action in this respect is the same. To get the full beneficial effects of either remedy it is necessary to give it in large and frequently repeated doses—twenty to thirty grains, at first every hour, and then every two, three, or four hours, as the symptoms decline. Salicylic acid and salicylate of soda cannot be given in such dose without some risk. Salicin may thus be given without fear.

The practical issue with which we have to deal is thus a very narrow one. Given two remedies which cure acute rheumatism with equal certainty and equal speed, but which, independently of their anti-rheumatic effect, exercise different actions on the system, which shall we prefer—that which has a tonic, or that which has a depressing action?—that which gives rise to no unpleasant effects, or that which may cause alarming, possibly fatal, depression? It may, indeed, be said that such large doses are not necessary. My answer is, that to get the full

beneficial effects of either salicin or salicylic acid in acute rheumatism, such large doses *are* necessary. By smaller doses—ten or fifteen grains every hour or every two hours—an attack of acute rheumatism may be arrested in two or three days. But let the remedy be given in the larger dose, and the process of the disease may be arrested in half the time. In a malady which tends to involve the heart and entail on the patient the terrible results of an endocarditis, every hour is of consequence. Cut the malady short in one day, and you may ward off cardiac complications which may appear if it lasts for two or three. It takes about an ounce of salicin or of salicylic acid to cure a case of acute rheumatism. The sooner this quantity is got into, or rather is passed through, the system the better. My practice now is to give thirty grains every hour. By the time that an ounce has been thus taken—that is, in sixteen hours—the patient is generally free from pain, and the temperature at or near the normal. I then give thirty grains every two or three hours till another ounce is consumed. After that thirty grains are given three times a day for a week or ten days, to guard against the possibility of relapse. Not unfrequently the patient feels better after three or four powders have been taken, and is practically out of the attack before the ounce is consumed. In such cases the interval between the doses may be widened after six or eight have been taken. Such is the course of events in favourable cases, and almost invariably their course in young subjects who have not previously suffered, or have done so only once or twice. In older subjects, who have had frequent and long-continued attacks, the acute symptoms may be as speedily allayed, but convalescence is more tardy and more apt to be interrupted. Cases treated by salicin seem to convalesce and pick up more quickly than those treated by salicylic acid or salicylate of soda.

Other of the salicyl compounds besides salicin and salicylic acid are available, and may prove of service. To only one of these would I now direct attention. Growing abundantly during the summer in our meadows, and by the sides of streams and ditches, is found the common meadow-sweet, the *Spiræa ulmaria*. The flowers of this plant contain a peculiar oil called *oleum spirææ*. This oil is salicylous acid. It is a slightly coloured mobile liquid. Taken alone or dissolved in spirit, it has a hot, pungent taste. Like salicylic acid, it causes some irritation of the throat when swallowed. From the few observations which I have made, I am disposed to think that an infusion of the flowers of the meadow-sweet may prove a serviceable remedy in rheumatism. As the plant will soon be in flower, I throw out the suggestion now in the hope that those who have the opportunity to do so may test its efficacy.—*Lancet*, June 21, 1879, p. 875.

9.—ON THE LOCAL TREATMENT OF CANCER.

By Dr. JAMES ARNOTT.

Four local remedies are employed in the treatment of external cancer—namely, excision, cauterisation, pressure, and cold. The only general or constitutional remedy requiring notice is opium. Before considering these local measures it may be well to advert to the yet undetermined point, whether they are all or any of them to be regarded as curative as well as palliative remedies. This is denied by those who, from believing that cancer is a constitutional affection, attribute every allegation of a permanent cure having been effected by a local remedy to an error of diagnosis, to mistaking an innocent for a malignant tumour. But few, it may be presumed, would assert that means of this kind may not remove local affections in cancer, just as certain means applied locally are capable of removing local affections in scrofula and other unquestionably constitutional diseases. Granting that the disease is constitutional, it proves fatal only by its local effects or manifestations.

Very few surgeons now resort to the excision of a cancerous tumour with the hope of thereby curing the disease, but many do so for other purposes. Life may be prolonged by it, and there will be a cessation of suffering during the time which elapses between the excision of the tumour and its recurrence—a period, however, which, in the greater number of cases, does not exceed six months.

The principal objection to excision being employed as a palliative is the danger from the operation itself. The mortality from this has generally been estimated at about 12 per cent., but the rate is much higher when large incisions are made with a view of giving relief of longer duration.

In those cases where, from peculiar circumstances, excision is desirable, this danger would be lessened by substituting congelation for chloroform in its performance, not so much on account of its safety as an anæsthetic, as from its being a preventive of erysipelas, which is the ordinary cause of a fatal issue; or if the incision is deep, and complete freedom from pain, is required, by using both. The regulation of the temperature of the wound by the current apparatus is another important precaution.

That the removal of cancerous growths from the breast by caustic is much safer than that effected by the knife, was satisfactorily proved by the extensive trial of this method some years ago at the Middlesex Hospital. The great objection to the treatment was the extreme and long enduring pain which it caused, but this (as I had afterwards the opportunity of showing at the same hospital) can be completely prevented by combining congelation with the caustic. By freezing a tumour for

more than an hour before applying chloride of zinc, the benumbing produced was so complete and lasting as to prevent pain during the whole time required by repeated cauterisation for its removal. The details of the process are given in a tract by the writer, published soon afterwards, and in Mr. Collis's work on Cancer.

The principal objection to pressure as a remedy of cancer has been the difficulty of applying it and of regulating its degree from time to time, according to the feelings of the patient. For the pressure of a bandage, employed by Young and Récamier, Dr. Neil Arnott substituted, and with great advantage, the equal and more easily controllable pressure made by a spring and a slack cushion of air. Its effect, according to Dr. Walshe, was "the total removal of the morbid production in the most favourable cases," and in others "the gradual reduction of bulky masses to small, hard, flat patches or rounded nodules, which appear to be, both locally and generally, perfectly innocuous."

It has been doubted whether the patches or nodules following the pressure are so innocuous as they have been represented, but the probability is that they are as inert as a bullet often is when bedded in the flesh, or as a permanent hard and bulky cicatrix. The following passage in Mr. Spencer Wells's work on Cancer bears on this question. "I saw," he says, "a lady a few weeks ago with a small tumour like a walnut in her breast, quite painless and not causing the least anxiety, which I saw nine years ago under the compression treatment; and it was then, to all appearance, a large cancerous tumour on the point of ulcerating. I have not the least doubt that if the tumour in that case had been removed, either by the knife, or by caustics, the patient would have been dead seven or eight years ago."

It must at first sight appear extraordinary that, with such satisfactory proof of its value, this method should not have been more frequently employed. The reason probably is, that although pressure generally relieves pain from cancer, at other times pain is caused by it, and this is more likely to happen if the morbid part is heated by the cushion, and if there is any difficulty of moderating the pressure when the patient's feelings render this desirable. By the pressure of water, applied as will be presently described, both these difficulties are surmounted, and on that account, especially at the beginning of the treatment, or while the parts are irritable, the fluid pressure should be preferred.

Cold of various degrees and duration has been employed in cancer, for the relief of the pain caused by it, for dispersing the growth, and in a protracted application of an intense degree, either alone or combined with caustic, for destroying it.

Professor Broca of Paris institutes in his work on tumours a minute inquiry into the origin of the use of cold in cancer, of which he gives the credit to the late eminent physician Dr. Hughes Bennett. It is a question of little importance, but having been brought forward by so distinguished a writer, it is necessary to mention some circumstances connected with it. In a treatise which I had published on the medical uses of heat and cold, an apparatus which has already been alluded to was described for attaining the important purpose of applying to any part of the surface of the body, and for any length of time, an uniform degree of temperature, combined, when it is desired, with any uniform degree of pressure. It consists of a thin waterproof cushion, through which, and two long flexible tubes connected with it, a slow current of water is made to pass from a capacious reservoir placed above it. In a review of the publication describing this apparatus, in the *Edinburgh Journal of Medical Science* for May, 1848 (of which Dr. Bennett was then the editor), its adoption is warmly recommended. It was of this apparatus that, two or three years afterwards, Dr. Bennett spoke, in his treatise on Cancer, as a means well adapted for its treatment by cold and pressure. He was the first to do so; I had not mentioned its use in this disease. The discovery that the tissues can be frozen without injury, and that congelation possesses remedial qualities in cancer and other diseases, was made at a later date. It is alluded to in the following extract from a subsequent work of Dr. Bennett:—"As a true anæsthetic or destroyer of local sensibility, congelation has been shown by Dr. James Arnott to be a most successful and manageable remedy. I have used it, in the way he has recommended, with excellent effect in a variety of local affections."

Dr. Bennett's suggestion of a moderate degree of cold in the treatment of cancer was founded on the plausible theory, that as cold restrains animal and vegetable growth, it would have a similar effect on the germs or cells of a cancerous tumour. My employment of congelation in this disease was merely an illustration of a statement in the "*Essay on Therapeutical Enquiry*" (page 34), that the extension of the use of a remedy which has proved effectual in one disease, to another of analogous character, is a principal source of improvement in practical medicine. I had had proof of its efficacy in other painful local affections. In my first employment of it in cancer, relief from excessive pain was the great object in view, but it did not seem improbable that some further benefit might be obtained, as from no other treatment are such palpable organic changes of a part produced, short of its destruction, as from congelation.

The benumbing effect of congelation, so valuable when it is

employed as an anæsthetic in operations, constitutes a powerful alleviative of suffering in external cancer. It has several advantages over the administration of opium. The action of congelation is confined to the diseased part, whereas the persistent use of large doses of opium greatly disturbs the functions of the brain and digestive organs. Many patients would rather endure considerable pain than the stupefaction and sickness produced by opium.

The relief from intense pain by opium is not so complete as that from congelation, and is far less durable. M. Broca, in his treatise just mentioned, adverts particularly to this long cessation of pain, inferring from it that some other organic change is produced by congelation than that of the vessels and nerves. He concludes his observations on the subject by the following statement:—"After the facts which have come to my knowledge, after those which I myself have observed, I am able to affirm that in many cases this method is a powerful alleviative of the pain which complicates cancerous and other tumours. It is a resource little used in France, but one which to me appears very valuable."

That congelation has been little used in England for this purpose is to be attributed to the difficulty of applying it in certain cases, especially when the uterus is the seat of the disease. The directions for this in the writer's treatise on Cancer have not always been attended to, and means have been employed quite incapable of effecting what is required. Amongst others, ether spray has been resorted to, which can only act as an anæsthetic when its evaporation is unimpeded. When a wide speculum can be introduced, permitting the application of a strong freezing mixture to the morbid part for a sufficient period, there will seldom be a failure; but when in the advanced stage of the disease this cannot be done, relief may still be obtained from a powerful frigorific solution passing as a stream through a double catheter of small size; and if the outer part of this catheter be covered by an inflatable membranous sheath, the solution will be confined to the inner part of the canal.

In congealing cancer of the breast or other outward parts, a small net of gauze or tulle may be used, containing the mixture; or, what answers much better, this may be applied by means of a gutta-percha cup fitted to the part. By the cup the smarting which might otherwise be complained of is in a great measure prevented, as the temperature can be very gradually reduced; and, if the cup is deep, pressure of any desired amount can be simultaneously made, either by the quantity of the mixture in the cup or by distending a bladder inserted in its upper part.

When cold has been employed for the dispersion of cancerous

growths, it has been applied either in an intense or congealing degree for short periods, or in a moderate degree for periods of considerable duration. The following remarks relate to the latter mode. The use of congelation for dispersion having been fully described in the author's treatise on Cancer, it is unnecessary to advert to it on the present occasion. The combination of congelation with caustic, and the prolonged congelation of cancerous tumours for the purpose of destroying them, are also the subjects of another publication.

In the fifth volume of the Transactions of the London Pathological Society, there is an interesting report by Mr. Simon of his use of ice in a case of cancer of the breast at St. Thomas's Hospital. The tumour was of the size of an orange, and caused constant and severe pain. By the application of ice in a bladder every day for an hour or two, there was a rapid decrease of its size; and when the patient left the hospital after a stay of thirty-four days it was no longer perceptible, the pain had entirely ceased, and her general health had improved.

In his treatise on the Diagnosis and Treatment of Cancer, the late eminent surgeon, Mr. Maurice Collis, relates the results of this treatment by ice in the wards of the Meath Hospital in Dublin—"If its use," he says, "be well borne by the patient, it should be kept up for days or even for weeks: it is surprising how small a cancerous breast will become under its persistent use. It is most useful in the more chronic cases; and is one of the most valuable aids we possess of producing or keeping up the atrophic form of cancer—the form to which we may apply the term of a natural cure."

The preference of a moderate degree of cold to congelation, by the writers referred to, appears to have proceeded from the idea that this degree was safer than congelation. But the contrary is the fact. A long-continued congelation might destroy the tissues, and unless the application of ice be carefully watched, these may be frozen without the patient or the surgeon being, for a long time, aware of it. Sloughing has sometimes occurred when ice has been applied in an unguarded manner in strangulated hernia, but never when congelation of short and limited duration has been employed for this purpose, which it has often been with success.

Instead of ice, it would be safer to apply, for a limited time and under careful inspection, a greater degree of cold, or one closely approaching congelation, and to repeat the application after short intervals.

Moderate cold and pressure may be used in cancer simultaneously or alternately. When employed together there is not only the increased remedial effect proceeding from a combination of their respective powers, but they also assist each

other's operation. The cold prevents irritation from pressure, and the pressure facilitates extension of the cold.

In concluding these observations on the application of cold in cancer, I cannot refrain from expressing regret that it has hitherto been so little employed either as a substitute for other plans of treatment in use, or by combination with them, as a corrector of their defects. But there is nothing singular in this delay. Few improvements in medicine have speedily overcome the opposition that has almost invariably attended their introduction. The cooling regimen in fever is an appropriate and striking example. The prejudice to congelation originating from the idea that it would destroy the tissues has long been overcome by its common use as an anæsthetic in operations; and that it would relieve excruciating pain from cancer by this anæsthetic property was so obvious as hardly to require confirmation from experience. Another impediment to its general use has been the fact that its efficient application is troublesome, and in some cases difficult. In requiring much care in some of its applications, it resembles the improved operation of ovariotomy and other surgical proceedings, which only prove successful when conducted with much pains. The importance of the object to be attained is surely a sufficient motive for taking this care in cancer, for in no other disease was improvement more required. The employment of cold in it is highly beneficial, while it is free from the objections to the ordinary remedial measures. In relieving pain it does not, like large doses of opium, quicken the progress of the disease by causing constitutional disturbance. It does not torture like caustic, nor put life in peril like excision.—*Medical Times and Gazette*, May 17, 1879, p. 533.

10.—ON AMYLOID DEGENERATION.

By GEORGE BUDD, Junior, Esq.

The term amyloid degeneration has been applied to the infiltration of human tissues with a substance which is in many respects the most curious compound known: a starch, yet yielding nitrogen on analysis; a proteid, perhaps, yet one which resists the action of gastric juice. I say perhaps, for it is the purpose of the present paper to discuss the nature of this compound, and to throw, it is hoped, some light upon its real character. To the term degeneration I still adhere, meaning to suggest, not the retrograde metamorphosis of tissues themselves, but rather the deterioration of some substance concerned normally in the nutritive process, whose altered state leads to its deposition in the tissues.

The cause of this condition has long remained a matter of

doubt. Dickinson regards amyloid substance as identical with dealkalised fibrin. He draws his conclusion mainly from the fact that it occurs commonly in connection with profuse supuration; but it would be nothing new in the history of medicine if it were some day shown that it was the cause rather than the effect of such pus-formations.

It first appears in the small nutrient arteries, and, after infiltrating their substance, pervades the tissues round about them. This would look as though some substance brought—habitually, perhaps—for the supply of the part had become in a way altered—degenerated, if you will—and deposited at its destination in such a condition that assimilation was no longer possible.

Do we know of any starchy compound concerned commonly in the nutrition of the body upon which we might look as the possible producer of amyloid material? Glycogen would seem to be such a substance. Secreted by the liver and stored up in that organ, it serves as a reserve fund of carbohydrate material for the body-supply. Through what changes it may pass before reaching the tissues, is a matter of doubt. It has been asserted that it becomes converted into sugar; but experiments upon this point have led to strangely opposite results. The stability of this glycogen is liable to great variation. In diabetes artificially produced, it is rendered exceedingly unstable, and is converted into sugar with great readiness. Does it not seem probable that there are other—it may be opposite—influences, such as would engender a contrary condition of things: a condition in which glycogen was rendered exceedingly stable—too stable, in fact, for proper assimilation? Glycogen in such a state, carried by the blood to its accustomed destination, would there remain, a stable product, assimilated only by the stronger tissues. There it would lie, and there accumulate, blocking up the small nutrient arteries, and impairing or destroying the function of the organ which they supply. Owing to its slow and gradual deposition in a finely divided state, we should expect to find it packing in its substance the remains of the attenuated tissues; giving externally reactions characteristic of glycogen, yet yielding nitrogen on analysis; resisting the action of gastric juice, since the proteids it contains are surrounded by a protective coating of glycogen.

It will be seen that our predictions would resolve themselves into a terse description of amyloid substance. Like glycogen, it yields, when treated with iodine, a dark mahogany colour; and this reaction is habitually used for its detection. Like glycogen, it is allied to starch, and turns blue when treated with sulphuric acid and iodine.

Analysed by Virchow, it was found, despite its starchy nature, to contain nitrogen: a circumstance which was at one time sup-

posed to depend upon its imperfect separation from surrounding tissues. Virchow's investigations have, however, been confirmed by subsequent analyses whose accuracy cannot be doubted; and I am not aware that the results are explicable on any other hypothesis than the one here set forth.

The following facts deserve notice in addition. Amyloid degeneration occurs most frequently in the liver; that organ is the manufactory and storehouse of glycogen. Cyon and Aladoff consider that the formation of sugar in artificial diabetes is directly dependent upon distension of the hepatic artery and its branches; that the artery, in fact, is intimately connected with the stability of glycogen. Curiously enough, amyloid degeneration always commences in, and is frequently confined to, that zone of the liver-lobule which corresponds to the distribution of the hepatic artery.

In conclusion, I would remark that the occurrence of diabetes and amyloid degeneration in the same individual by no means negatives the theory here laid down; for concentric deposits of phosphates and uric acid—substances indicating opposite conditions of the system—are not unfrequently found in the bladder. —*British Medical Journal*, May 3, 1879, p. 659.

DISEASES OF THE NERVOUS SYSTEM.

11.—ON THE TREATMENT OF CHOREA.

By Dr. W. H. DAY, Physician to the Samaritan Hospital
for Women and Children.

With regard to the treatment of Chorea, rest in bed is the first and most important step to observe. In many cases drugs exert only a secondary influence, rest, warmth, and proper food being all that are required; but the class is by no means small in which iron, quinine, arsenic, phosphorus, and strychnia fail as remedial agents. I have given sixteen drachms of the succus conii in twenty-four hours, to a girl ten years of age, without producing dimness of vision or dilatation of the pupil; indeed, the patient was no more affected than if she had taken water only.

Chloral hydrate has been recommended in large doses in violent chorea. The principle of treatment was to give thirty grains, and to repeat the dose, or half of it, if the patient did not obtain ten hours' sound sleep in the twenty-four. On waking, a second dose was given in proportion to the ascertained effect, but always less than the first. On waking again another dose less than the second, and so on till the amount of sleep had been obtained, when the chloral was discontinued till the next night. Of two patients so treated, aged eighteen and twenty, one was completely cured in one day, and the other on the fourth day. In

a case of acute chorea in a girl of nine, I found that five grains given every night produced tranquil sleep, and it was unnecessary to continue the drug beyond a week. In another case a girl, thirteen years of age, suffering from most severe chorea, began to take ten grains every four hours on admission, as she was much exhausted, and the mother stated she had not slept for a week. In the first twenty-four hours after commencing the drug she did not obtain more than two hours' sleep; then it was given every two hours. After following this treatment for another twenty-four hours, my report says, the effect of the chloral has been to induce sleep for ten minutes at a time, but the least noise woke her. The effect has also been to raise a small weak pulse from 60 to 72 and 76 per minute, and the respirations to 20. Towards the close of the day her sleep became so sound that the eyelids could be moved upwards and downwards for some seconds before reflex action was excited; then she would screw up the eyelids, and relapse into sound and heavy sleep for an hour. The remedy was gradually discontinued as natural sleep returned, and the cure was completed by large doses of sulphate of zinc.

There can be no question whatever that hydrate of chloral is a valuable remedy in some cases of chorea, particularly in those where vascular excitement is present and the pulse is good. Dr. Althaus considers that the theory of chorea is explained by active hyperæmia of the corpora striata and the parts surrounding the fissure of Sylvius, and that the beneficial action of hydrate of chloral is to be attributed to the anæmia which it produces in these structures. Its danger as a depressant is nothing compared to the repose and rest which it ensures to the nervous system, lessening as it does in suitable doses the extreme agitation of the limbs, and the violence of the choreic movements. Sleep so obtained gives the necessary time for repair to the over-excited parts, and will be found to succeed when morphia yields no result.

Dr. Drummond, of Newcastle-on-Tyne, cured an obstinate case of chorea, in a girl seven years of age, by the subcutaneous injection of *curara*. He commenced with an aqueous solution of gr. 1-40 for two days, increasing the dose on the third day to gr. 1-20, and the next day to gr. 1-10, on the fifth day to gr. 1-8, and on the sixth day gr. 1-5, by which time the patient had recovered complete power over the voluntary muscles. Two days later gr. 1-4 was administered, and there was no return. (Brit. Medical Journal, June 15, 1878, p. 857.) In a chronic case of chorea which was admitted into the Samaritan Hospital, under my care, in October, 1878, I determined to try the effect of *curara*. The patient was a girl eleven years of age, and had been under my care on three previous occasions with the same

disease. There was incessant agitation of the arms and legs, and it was necessary to keep her in bed. The heart's action was rather excited and thumping, and there was a soft systolic bruit over the apex. After taking hypophosphite of soda and iron, as well as cod-liver oil, she was not manifestly better, and any excitement or conversation would make her very fidgety and increase the muscular movements.

On the 15th of October I injected into the right forearm gr. 1-60 of curara with the following effect:—

16th. Ten a.m., no effect; 11 a.m., gr. 1-40 injected; 2-30 p.m., no change, pulse 72, gr. 1-30 injected; 6-45, since the injection she has been much quieter and is lying perfectly still, with complete command over the limbs; pulse 80, inclined to sleep.

17th. She passed an excellent night, and slept better than she had done for some time past, but agitation was returning in the arms, and I now injected gr. 1-20 at 10-45; at 6-45, as there was no further improvement, I injected gr. 1-10.

20th. No injection used to-day, but after 2 p.m. the limbs became more agitated, and the facial muscles were more active.

21st. The mouth, hands, and legs were in greater motion. The effect of the curara has been to keep her quiet for twenty-four hours, and then it has passed off.

I must admit that this drug fully answered my expectations, and I should be disposed to employ it again when the agitation is great, because it controlled the movements, and caused neither headache, sickness, nor any unpleasant symptoms. One difficulty is the alarm which the injection causes.

Lastly, I should like to say a few words about *sulphate of zinc*, and what I have noticed concerning its action. Small doses are sometimes utterly useless, when large doses succeed; and if it is determined to try the remedy at all, it should not be set aside till the latter have had a fair trial. I have given this drug in doses of from one to five grains three times a day, and continued it for a week without producing any effects, and the remedy has so repeatedly disappointed me that for some time I ceased to employ it. This most likely arose from giving it in too small a dose. Sir T. Watson gave it successfully in ten-grain doses three times a day, in a severe case which had resisted other remedies. There can be no doubt that zinc sometimes succeeds where iron and other remedies fail. In prescribing it, the dose should not exceed a grain three times a day to begin with, and should be gradually increased till there is nausea, or an amelioration of the symptoms. In a chronic case which was temporarily relieved by the hypodermic injection of curara, I began with two-grain doses twice a day, increasing the dose daily, till on the ninth day the patient was taking 18 grains.

For the first time this controlled the muscular agitation, improved her voice and appearance, and caused no sickness. On the tenth day, she took twenty grains three times a day, and on the twelfth day forty grains twice a day, without causing the least unpleasant symptom. The heart on admission was rather unsteady, with a soft apex bruit, which I attributed to debility, it was now quiet and regular, and the murmur had entirely disappeared. In another similar case no benefit resulted from large doses of sulphate of zinc, and the patient only became slightly sick when taking ninety-six grains daily in three doses.

I must urge, in conclusion, that the more we look at chorea from the neurol side, the more we realise its origin in anæmia, debility, and all sources of exhaustion, the more successfully shall we be able to control and to cure it. I believe that a blind credulity in its rheumatic origin, of which we still hear so much, is a serious mistake to entertain, because it induces us to overlook a cause of far greater frequency, and leads to a line of treatment which I have satisfied myself on several occasions, has further tended to impoverish the blood and aggravate the irritability of the nervous system.—*Medical Press and Circular*, May 21, 1879, p. 399.

12.—CASE OF TRAUMATIC TETANUS, TREATED WITH THE
HYPODERMIC INJECTION OF ATROPIA; AMPUTATION
OF GREAT TOE; RECOVERY.

By Surgeon D. H. CULLIMORE, Ex-Residency Surgeon at the
Court of the King of Burmah.

In April, 1875, when the highest temperature in the shade was 98° F., with a great diurnal variation, the rainy season having just set in, a Lascar, a camp follower, was admitted into the hospital for details at Rangoon, Burmah, suffering from a lacerated contused wound of the great toe, inflicted some three days previously by the tread of a horse. The patient (a powerful muscular man, aged twenty-eight years) was confined to bed, and a rice poultice, medicated with laudanum, applied for two days, when, no improvement taking place, and the condition and seat of the injury being such as is frequently followed by tetanus, the toe was amputated, with the object of removing what I was afraid might become the exciting cause of that disease, when, somewhat to my surprise, in about fifty hours after the performance of the operation, the symptoms of tetanus became manifest—first, by yawning and listlessness; secondly, by stiffness of the muscles of the neck and the abdomen, accompanied by the usual expression of face; and, lastly, and later on, by spasmodic contraction of the abdominal muscles and opisthotonos, which latter continued for

about six hours on the fourth day from the inception of the disease.

As this was the fourth case that came under my notice in the space of six months, one of which was treated with chloral, and the others with hypodermic injection of atropia in combination with morphia, and as all three terminated fatally between the ninth and the twelfth day, I felt convinced that chloral would prove a failure, and, judging from the physiological effect of morphia and atropia, which is the reverse of each other, at least in so far as their action on the pupil is observed, I determined to try atropia alone, more with the object of noting its action than with any but a vague hope that it might be the means of preventing a fatal issue. On the first appearance of the symptoms one-sixtieth of a grain of atropia was injected hypodermically over the dorsal spinal region, and was repeated three times daily. On the morning of the second day one-fortieth of a grain was injected every four hours, and continued for six successive days, till the spasms had entirely ceased, and the stiffness disappeared from all but the muscles of the neck and face, which, as they were the first to become affected, continued longest under the influence of the disease. On the eighth and ninth days the dose was reduced to one-sixtieth of a grain twice a day, and subsequently reduced to one-sixtieth of a grain at night for a further period of two days, ending on the evening of the eleventh day from the commencement of the disease, when the patient, though not yet cured, was well out of danger, and in a fair way to recovery.

I should have mentioned that the bowels were constipated throughout, and were acted upon by four grains of calomel with forty grains of compound jalap powder, administered every other day, and that after each evacuation the patient invariably expressed himself "much lighter" and relieved.

Under the influence of tonics and nourishing diet, with an occasional purgative, such progress was made that on the twenty-sixth day from his admission into hospital, and on the twenty-first from the manifestation of tetanic symptoms, the man was discharged. During his stay in hospital, and within the space of nine days, two grains of atropia were introduced into his system, which caused neither dilatation of the pupil nor any continued increase of temperature; in fact there was no ascertainable physiological action, with, perhaps, the exception of drowsiness and slight occasional hyperæsthesia of the surface, which I am now more inclined to connect with the disease than the remedy.

Though this case occurred so long ago, the facts may be relied upon, as the notes were taken at the time, but not published from want of leisure.

Remarks.—The points illustrated by this case are :—

1. That tetanus—i. e., a series of reflex phenomena depending upon an over-excited or congested state of the brain, the spinal cord, and their membranes—is capable of being relieved, or even cured, by atropia, when administered in comparatively small doses, extended over a certain period of time according to the severity of the symptoms; though we know from the experience and experiments of Drs. Harley, Fraser, and others, that when given to its full physiological effect it produces excitement and congestion of the cord, followed by the usual reflex results, as jactitation, muscular spasm, and convulsive fits.

2. That the administration of the medicine was not followed by any of the easily recognisable symptoms of the drug (two grains of which has caused the death of a healthy adult when given in one dose), proving both the tolerance induced by the disease, and, perhaps, also illustrating the homœopathic theory or formula, *sine* the infinitesimal system of dosage.

3. That amputation of the injured part, recommended so strongly by Larrey and others, even after the supervention of tetanus, though it may perhaps help to lessen the severity of the disease, does not act as a prophylactic, and should, I think, never be had recourse to after the symptoms have declared themselves. It would then be injurious, for the peripheral irritation would have become central, and independently dynamic. For the same reason, the division of nerves should not be resorted to. In two cases where I examined the nerves after death I failed to perceive that they differed in any way from those of the opposite side. In one of these there was slight congestion of the membranes and a softening of the cord in the lumbar region, and in the other a peculiar cloudiness of the cord, which may, however, have been due to post-mortem changes. Yet it is certain that there is some lesion, though in every case we may not be able to perceive it. This lesion should be looked for in that portion of the spinal cord with which the nerves from the affected part first communicate.

4. If the line of treatment adopted in this case should be found beneficial in others of the same disease, I would suggest that it might be extended, with such modifications as may be necessary, to the treatment of such allied diseases as epilepsy, puerperal convulsions, and hydrophobia.—*Lancet*, July 12, 1879, p. 42.

DISEASES OF THE ORGANS OF CIRCULATION.

13.—ON BLOOD-CLOTTING IN THE VESSELS.

By AUGUSTUS J. PEPPER, Esq., M.S., F.R.C.S.

There is no more intricate problem awaiting solution at the

hands of the clinical and pathological investigator than that of clotting of the blood in the vessels during life. Both as to the causes of the phenomenon and the subsequent changes, the most discrepant opinions have been entertained; each observer founding his own particular view upon some supposed facts met with in the history of this morbid state—facts not unfrequently mistaken in themselves, and inferences therefrom equally erroneous. Now we cannot be so very much surprised at this when we call to mind the many explanations that have been given by the most painstaking and competent authorities of the why and wherefore of coagulation of the blood removed from the body—e.g., the now discarded theories, firstly, of simple change of state of fibrin existing as such in the blood, and secondly, that of Dr. B. W. Richardson, who ascribed it to the escape of ammonia.

Before discussing the subject matter of my paper, I may perhaps be allowed to make a few remarks on the present state of our knowledge concerning the causes of coagulation of the blood drawn from the vessels. This is due to the interaction of three bodies—fibrinoplastin, fibrinogen, and a ferment, resulting in the combination of the two former; that is to say, fibrin which does not exist in the fluid blood comes to be formed under certain ascertainable conditions as the latter becomes solid. There is, so to speak, a constant antagonism between two sets of influences which tend in opposite directions—between those that favour and those that retard or prevent coagulation. Among the former are—1, rapid motion, as in whipping, or, in other words, multiplication of points of contact; 2, exposure to not-living matter. Among the latter we have—1, the addition of a third of the bulk of a saturated solution of some neutral salt; 2, a temperature of about 32° F.; 3, moderate heat; 4, the abstraction of hæmoglobin; 5, the absence of a neutral salt, whose presence, according to Schmidt, is essential to coagulation.

Now blood, whilst in the vessels, is known to clot more easily when the current is slowed, just the reverse of what is found in shed blood; but this is only an apparent anomaly, for when we consider that slowing of the blood-current means a diminution in the nutrition of the coats of the vessel, and that this diminution of nutrition brings it to a state of lower vitality, the question of slowness or rapidity of flow is resolved into that of contact with foreign matter.

As to the so-called ferment—whether it be fluid, or solid in the shape of germs visible or invisible—few can doubt the conditions under which what are termed the consequences of its action are brought about in the living body, and none can conscientiously deny the advisability of doing all that lies in our

power to counteract its influence, which may be so potent for evil. And it is here, I think, that the antiseptic treatment of wounds shows itself to manifest advantage, for when decomposition of the discharges is allowed to take place unchecked, how much more likely are we to get thrombosis extending beyond the immediate seat of injury, how much more likely in such cases to get rapid softening of the clot, and, as a natural consequence, detachment of emboli rife with mischief, by reason of their mechanical action, and still more so because they are saturated with septic matter. It is often a question of pyæmia or not—of life or death to the patient.

The two prominent features of thrombosis are:—1. Its causation. 2. The fate of the clot.

To gain some clue to an answer to the question, Why does the blood coagulate in the vessels? let us consider the diseased states which are not unfrequently complicated with the untoward event. And firstly, because they will be fresh in the minds of many of you, I will give you the fatal cases that happened in our hospital during the past year. There were four of them. The index of the pathological reports runs practically as follows:—

No. 1. Female, aged thirty-eight.—Phthisis; tubercular ulceration of intestines; perforation of cæcum; peri-cæcal abscess; thrombosis of right common and external iliacs and femoral veins.

No. 2. Female, aged forty-six.—Fracture of ribs and pelvis; localised pleurisy; decolorised granular clot in right ventricle; embolism and subsequent thrombosis of pulmonary artery.

No. 3. Female, aged forty-three.—Primary thrombosis of iliac, femoral, and popliteal arteries and veins; gangrene of leg; bed sore; recent infarction in liver.

No. 4. Female, aged sixty-three.—Cancer of cæcum; perforation; acute peritonitis; phthisis; thrombosis of left femoral and popliteal arteries and veins; gangrene of leg.

The third case, since the patient was apparently a healthy woman until the onset of the plugging, seems to point to the condition of the walls of the vessels as the immediate cause; and the microscopical appearance of the internal coat of the arteries gave weight to that view, even if it did not establish its truth.

In the other three cases, you note that in two of them there was a combination of circumstances likely to lead to a vitiated state of blood: in one cancer and phthisis; in another phthisis and ulceration of the bowel, with abscess from perforation; whilst the remaining one falls into the same category, seeing that in it there was sudden diminution of bodily activity through confinement to bed from severe injury.

Amongst those that have come under my own observation are the following:—

Thrombosis of cerebral veins in phthisis and in marasmus; of the basilic and other veins of the arm in septicæmia, from compound fracture of jaw, with cellulitis of forearm; of pulmonary artery, fatal immediately after parturition; of the varicose veins of the lower extremity; of axillary and femoral veins in post-parturient women; of the axillary vein in rheumatic fever; of the brachial artery from bruising. In textbooks you will find, amplifying the list: Plugging of the cerebral arteries in syphilis; capillary thrombosis as the starting-point of cancrum oris, and as the explanation of the multiple openings in carbuncle; not forgetting those cases where coagulation is intentionally brought about, as in the ligation of arteries, and the operation for varicose veins. On analysing the foregoing, we arrive at the conclusion that there are two diseased conditions—one of the bloodvessels, the other of the blood,—which are so frequently seen as marked features in thrombosis that we are driven to accept that a casual relation exists between them. Slowing of the blood-current from weak action of the heart acts as a subsidiary factor.

In the first place, let me explain the different ways by which the blood may become so vitiated that its fluidity can no longer be maintained; and the simplest way of doing this is to make two groups: (*a*) that in which the fibrin factors are increased; and (*b*) that in which excrementitious matter is in excess from defective elimination, as in gout, or abnormal material is introduced from without, as in septicæmia.

The two groups are purely artificial, and are introduced rather for the sake of clearness of demonstration than for clinical accuracy. If it be admitted that the composition of the blood does play a very important part in its own coagulation, there remains for consideration the nature of the changes which directly underlie the phenomenon. The reasoning on this point, since we know of so few facts and still less of their correlation, is to a great extent speculative. Yet although this forbids our drawing any wide conclusions, there is sufficient that is probable to urge to further research and to some extent to indicate the line of action to be taken. Such questions as the following suggest themselves:—Is the chemical composition or constitution of the fibrin factors altered? If so, does this depend on a general physico-chemical change of the constituents of the blood in which they are involved, or is there some definite yet subtle agency at work sufficing to effect the necessary means to the end? And supposing this to be the fact, what is the nature of that agency? Is it an altered mode of life of the white corpuscles

by virtue of which they entail in their own changes the coagulation of the blood? We know that on watching the clotting process in liquor sanguinis obtained by subsidence, it is seen to commence by the formation of fine threads of fibrin in connexion with the leucocytes. Or, again, may not the conditions of these leucocytes be one of death rather than life, and that thus they count only as any other foreign matter? By most observers the former hypothesis is deemed the correct one; but Mr. Wharton Jones, one of the greatest of living authorities on the subject, looks upon the amœboid movements as the manifestations only of moribund corpuscles. Lastly, does not the whole question turn on the generation of a ferment, and thus place us on a less equivocal basis by relegating the condition to others of a similar nature tolerably well understood? The fact that coagulation of the blood is more likely to take place in such diseases as pyæmia, septicæmia, and the like, where minute organisms called micrococci have been found, lends weight to such a theory.

Since the above lines were written there has been a case in the hospital of acute necrosis of the clavicle, in a well-nourished girl aged fourteen years, coming on without apparent cause, and this was the sequence of events: acute periostitis and osteitis leading to necrosis, thrombosis of the common femoral vein, disintegration of the clot, embolism, and circumscribed metastatic abscesses in the lung; at no time was there a wound of any kind. This case illustrates two points in pathology: firstly, that the blood-poisoning may be developed without access of air to favour or account for the necessary decompositions—a proof, I think, that the most virulent pyæmia may depend entirely on the infective matter resulting from the inflammatory state; and, secondly, that the change in the blood is sufficient to bring about its coagulation without appreciable disease in the walls of the containing vessel.—*Lancet*, June 7, 1879, p. 803.

14.—ON THE SIGNIFICANCE OF THROMBOSIS OR BLOOD-CLOTTING.

By Dr. ROBERT BARNES, Obstetric Physician to St. George's Hospital; Censor of the Royal College of Surgeons.

The study of pathology through obstetrics is constantly presenting us with observations which have nearly all the conditions of simple physiological or laboratory experiments. We start with a healthy subject. The induction of pregnancy—still a physiological condition—is the first step in the experiment. Then we observe the changes wrought in the blood, in the nervous system, and on the various organs and their func-

tions under this new condition. We see the system is put upon its trial. If sound, and if no heterogenetic morbid element be introduced, the woman comes out of her trial unscathed. All returns to the ordinary physiological state. But we know too well that the changes wrought in the constitution of the blood, and in the nervous centres, in many cases approach so closely to the border-line between physiology and pathology, that a very slight accident will precipitate the transition from health to disease. Now, the course of study indicated is: first, to take note of the conditions induced by pregnancy; secondly, to seek for the accidental conditions supervening upon the pregnancy; thirdly, to trace the succession of phenomena resulting.

I now propose to cast a very short and superficial glance at the significance of thrombosis, limiting myself to one problem only, purposely excluding embolism. Thrombosis arises in the course of many diseases, acute and chronic; and, in many of these cases, the antecedent conditions and immediate causes have been traced with consummate skill to brilliant success. I need hardly refer to the researches of Virchow, Cohnheim, Richardson, Schiff, and many other pathologists.

We need not go into minute differential analyses of the blood in the non-pregnant and in the pregnant woman. The main distinctions, traced by Andral and Gavarret, are well established. It will be enough for our purpose to call to mind that, in pregnancy, the blood is more watery, contains a smaller proportion of red corpuscles, usually a greater number of white globules, and that it is constantly loaded with a larger proportion of fibrin, which must be regarded as an excrementitious element. To this general description, we may add that the blood is also liable to constant invasion of waste products of nutrition, coming from the mother's own system and from that of the embryo. These waste products will exist in variable proportions at different times, according to variations in diet, exercise, in the rapidity of nutritive processes, and in the working powers of the secreting and excreting organs. So long as the lungs, heart, and the glandular system are in good working order, the excrementitious matters are easily eliminated, and the healthy balance is maintained. But so soon as the eliminating part of the organism is impaired in its action, there will be a gathering of noxious materials in the circulation, the first effect of which will be exerted upon the blood itself. The blood, in short, is poisoned. It is chemically affected. And, according to the nature and quantity of the noxious material and the state of the system at large, or of particular organs, and no doubt also according, in some cases, to inherited diatheses, different reactions will ensue, assuming more or less definite pathological features.

We may assume, in passing, that in all these cases of blood-poisoning the circulation is affected dynamically: that is, the heart and bloodvessels act with greater rapidity and difficulty; and also that the nervous system is more or less disturbed. But it is enough, at present, to limit our attention mainly to the blood itself. It is well known to those—they are not, however, many at the present day—who have bled pregnant women, how readily the blood drawn coagulates, and how marked is the cupping and buffing. This is due to the excess of fibrin, resembling in this respect the blood of rheumatic fever. Prone-ness to clot, then, is the ordinary character of the blood in pregnancy. It is more disposed to clot in the vessels than is the blood of non-pregnant women. This is the first factor in the problem. What are the subsequent factors? Blood out of the body clots; 1, because it is exposed to the air; 2, because it is no longer kept moving as it is whilst in its proper vessels; and probably under other influences.

Another question is, At what time does thrombosis usually occur? Clinical experience answers this question very decidedly. In by far the greater number of cases, thrombosis declares itself after labour, and generally after the lapse of several days. This will lead us to inquire: What are the new conditions left by labour and arising during the puerperal process? We may note, first, an almost sudden diminution of nerve-tension. There is a revulsion in the psychical condition; a change in the spinal system, of which the most striking feature is the lowering of the reflex or diastaltic irritability. Corresponding with this, we find a marked diminution of the vascular tension. The sphygmograph, which before labour showed tension as high as that attending advanced Bright's disease, quickly gives a tracing of a quite different character. The frequency of the heart's action is considerably lowered. This lowering of nervous and vascular tension takes place very quickly after labour. Intimately related to this lowered nervous and vascular tension are two events: 1. The sudden cessation of the great developmental work going on in the uterus which had called for the exalted tension and increased quantity of blood; 2. The shedding of a greater or less proportion of this increased quantity of blood. There is always some hemorrhage, and not seldom, as we know too well, a profuse, even dangerous, loss.

This sudden drain of blood induces enormous activity of absorption. And absorption is not always discriminative. It takes up noxious matter as well as healthy.

But the blood is not materially altered in its constitution until after two or three days have elapsed; that is, until the process of involution of the uterus and of other structures which had undergone a special development to fit them for the work of

parturition has set in. Then there is a rapid inpouring of effete matter into the circulation, a sudden strain thrown upon the glandular system to discharge it, and a diversion of vascular developmental activity from the pelvis to the breasts. The breasts thus perform a double duty. As well as secreting food for the infant, they act as excreting organs, and by keeping all the functions in harmonious balance. If the action of the breasts be suppressed, more work is thrown upon the other special excreting organs; and there is the greater probability of the accumulation of noxious matters in the blood. Accordingly we see that, in women who will not, or who cannot, suckle, puerperal complications, as fever, pelvic inflammations and mania, are far more frequent.

Now, what is the relation of these facts to the problem immediately before us, the theory of thrombosis? Why, in the first place, is thrombosis so very rare during pregnancy? At this time, the excess of fibrin in the blood is at its maximum. It is highly coagulable. Why does it not coagulate? Something is wanting to precipitate the fibrin. This something we find in the sudden irruption of effete matter, and in the product of involution, a process which does not begin until after labour. It is in excess of that quantity which the excretory organs can discharge. If this excess be very great and acting upon blood, perhaps deficient in coagulating power, the whole circulating mass is invaded: and septicæmic puerperal fever is produced. But, if the excess be in moderate proportion, it is met by quick coagulation near the seat of ingestion, and thus is prevented from seriously affecting the general mass of blood. Thrombosis thus is a conservative process. Precisely the same order of events is seen in the lymph and lymphatics which play so active a part in absorbing the waste products of uterine involution. But the lymphatic glands further aid the conservative process of thrombosis by delaying the effete matter in its passage towards the heart, and by subjecting it probably to some kind of metamorphosis which deprives it of its noxious properties. Healthy thrombosis or phlegmasia dolens may follow the simple entry of the ordinary physiological fatty *débris* of the superfluous uterine tissue. But in a large proportion of cases the precipitating agent is something more decidedly noxious. If the uterus have contracted imperfectly, if any bits of placenta or decidua remain adherent, if clots be retained, then, decomposition or disintegration going on in the cavity of the uterus and in its sinuses, there is a direct supply of septic irritating matter. This, entering the veins and lymphatics in the broad ligaments and the internal iliacs, causes coagulation. This coagulation serves as a secondary barrier against invasion of the circulation at large; the uterus, which by firm contraction should be the first barrier, having

failed. The thrombosis in the pelvic vessels is probably favoured also by the comparative slowing or retardation of the blood in them, due in some measure to the pressure of the enlarged uterus upon them. But there are many facts which point to the contact of septic matter with the blood as the immediate or efficient cause. The experiments of Gaspard, Henry Lee, and others show that the injection of septic matter into the living blood is a sufficient cause. Clinical observation adds confirmatory facts.

The entry of septic matter into the blood is notoriously attended by signs of constitutional shock; the most prominent of which are rigors followed by fever, and next by attempts at elimination. Rigors may generally be observed as the precursors of thrombosis. In one case especially, I watched a succession of thromboses, and every one was ushered in by shivering. This probably means that some of the noxious matter makes its way into the general mass of blood, breaking through the protecting barriers of the venous thrombosis and the lymphatic glands. The repetition or extension of thrombosis may sometimes be stopped by washing out the uterus, and especially by the use of quinine, ergot, and other agents which provoke the uterus to contract. Thus the supply of septic matter is cut off. We see analogous facts in the course of malignant disease of the uterus. Thrombosis of the pelvic veins is not uncommon towards the end of this disease. It is never, at least so far as I have seen, observed before the stage of ulceration has begun. Until this has begun, the subject may exhibit all the outward marks of blooming health. But when it has set in, that is, when a raw surface is covered by decomposing necrosed tissue and foul discharges, absorption takes place, and the blood in the pelvic vessels may coagulate. Before this event, we have evidence of blood-empoisonment in the so-called cancerous aspect and cachexia. Now, I have seen the cachexia and thrombosis so arising gradually removed after amputation of the offending mass, thus substituting a comparatively clean granulating surface for that which yielded the foul empoisoning material. And I have seen, when ulceration and necrosis have returned, as return they will, a renewal of the thrombosis and cachexia.

Here, too, the thrombotic process is probably favoured by the stagnation of the blood in the vessels of the broad ligament, caused by the matting of all the tissues by the spread of the disease and by the attendant inflammatory exudations. And the antecedent hemorrhages and other conditions have induced the hyperinosis of the blood which is so important a condition of clotting and so conducive to absorption. There is another illustration of this local septic origin of phlegmasia dolens, of which I have seen several interesting examples.

In two cases of ovariectomy in which I tied the pedicle with strong whipcord and returned it into the peritoneal cavity, an inflammatory swelling arose after a few days in the iliac fossa of the pedicle side, and following upon this some degree of febrile irritation, and then phlegmasia dolens of the same side was developed. These cases were conducted under the antiseptic method. The phlegmasia dolens gradually subsided. Both made good recoveries.

In rheumatic fever, we find another illustrative example. Indeed, this disease and pregnancy present, perhaps, the two most striking examples of quickly induced hyperinosis. In rheumatic fever, then, we have large excess of fibrin. To cause coagulation, a chemical irritant is wanted. This is found in the acid or other products retained in the blood. Fibrin becomes precipitated on the valves of the heart.

One condition seems common to most of the forms of thrombosis: that is, more or less sudden overpowering of the excreting organs, involving retention of matters that should be ejected. Thus, we frequently observe as an immediate cause of thrombosis, suppressed action of the skin from exposure to cold; and the thrombosis which in some cases follows quickly on physical and mental shock may be explained by the sudden arrest or disturbance of glandular action.

Cruveilhier distinguished two forms of what we now call thrombosis: 1. Adhesive phlegmasia; 2. Suppurative phlegmasia. In the first or adhesive form, we see the healthy conservative process of thrombosis blocking the entry of poisonous matter into the general mass of the blood and sequestering it in the veins invaded. In the second or suppurative form, we see the failure of the conservative or blocking process. There is thrombosis, but the clotting is imperfect. This is generally the case in the thrombosis which precedes death from cancer of the uterus. Wanting in firmness, probably from the beginning, in fatal cases (and it is most commonly fatal) the clots are found soft, pultaceous, easily breaking down, running into puriform fluid, and often extending far down the femoral veins and high up the vena cava. These cases are characterised by an earthy aspect, low fever, and general depression; in these respects contrasting with the general condition observed in the conservative form. The local characters are not less distinct. The conditions of the affected limb in pure phlegmasia dolens are well known. The leg is tense, greatly swollen, firm, resisting pitting, white, and shining. In the unhealthy "suppurative" form all is different. The leg is swollen, it is puffy, doughy, pitting on pressure, the skin is dull in colour, and it is no tense enough to shine.

The first form tends to recovery ; the second to death. In the first form, the great glandular organs are healthy ; in the second, they will often be found diseased ; that is, the liver and kidneys are affected with lardaceous or other chronic degeneration. These degenerations or other imperfections of the liver and kidneys are a chief cause of the mortality of puerperal fevers, and of some cognate deaths after ovariotomy.

I will not intrude upon you other analogies. Many will present themselves to your minds. I will only hint at the close pathological relations between these cases of so-called phlegmasia dolens in childbed women and septicæmic puerperal fever, pelvic cellulitis, and pelvic peritonitis and inflammations of other serous membranes.

Phlegmasia dolens may be taken as the type of what I long ago proposed to call the "autogenetic" puerperal fever, in contradistinction from those fevers which owe their origin to empoisonment from without, the "heterogenetic" fevers. But we must not forget that phlegmasia dolens may ensue upon the ingestion of foreign poisons.

The great clinical lessons illustrated and enforced by the very imperfect remarks which I have been invited to submit to you are these.

1. The origin of phlegmasia dolens in lying-in women can mostly be accounted for by processes springing up in her own system.

2. The blood of the recently delivered woman is in a state highly prone to coagulate.

3. It will coagulate when it is invaded by effete materials or septic matter in undue proportion to the excretory power of the system.

4. Such undue proportion will accumulate when the free action of the great excretory organs, the breasts, lungs, liver, kidney, skin, and mucous membrane of the intestines is greatly impaired by chronic antecedent imperfection, or is suddenly checked under the influence of cold, emotion, or other form of shock.

To anticipate these causes, to prepare and to keep the glandular system in good working order, to prevent the accumulation of poisonous matter in the blood, is the obvious indication, one which we ought to be able in most instances to carry out.

The theories or hypotheses of thrombosis arising under conditions other than puerperal must be in harmony with what is observed in puerperal thrombosis.—*British Medical Journal*, June 28, 1879, p. 966.

15.—THE SPHYGMOPHONE.

AN INVENTION OF A METHOD FOR MAKING THE MOVEMENTS OF THE PULSE AUDIBLE BY THE TELEPHONE.

By Dr. BENJAMIN W. RICHARDSON, LL.D., F.R.S.

While experimenting with the audiometer, it occurred to me that I might get a secondary or telephonic sound from the movements of the pulse at the wrist. I have effected this in a very simple manner, by adding a microphone to a Pond's sphygmograph. I mount on a slip of talc, glass, wood, or ebonite, a small plate of metal, such as platinum, or a little bar of gas carbon. I place the slip in the sphygmograph as if about to take a tracing of the pulse. I connect one terminal from a Leclanché's battery to the metal or carbon, and the second terminal from the cell to a terminal of the telephone. Then I connect the other terminal of the telephone with the metal rod of the sphygmograph which supports the slip. The instrument is now ready for use. It is placed on the pulse in the ordinary way, and is adjusted, with the writing needle thrown back, until a good pulsating movement of the needle is secured. When the movement is in full action, the needle is thrown over to touch the metal or carbon plate, which it traverses with each pulse-movement, and completes the connection between the telephone and the battery. The needle, in passing over the plate, causes a distinct series of sounds from the telephone, which correspond with the movements of the pulse. When all is neatly adjusted, the sounds heard are three in number, one long sound and two short, corresponding to the systolic push, the arterial recoil, and the valvular check. The sounds are singular, as resembling the two words, "bother it." The sounds can be made very loud by increasing the battery power.

This little instrument is not a permanent recorder of the pulse like the sphygmograph, but it may be made very useful in class, for illustrating to a large number of students at one time the movements of the natural pulse and the variations which occur in disease. I call the invention the sphygmophone.

Additional Notes, May 26, 1879.—On the whole, the carbon plate answers best, and for all ordinary purposes two Leclanché's cells are sufficient. From them the sounds produced are audible a distance of forty feet. By extension of the telephone wires the observations may be made in a room at a distance from that which contains the person whose pulse is yielding the movements that produce the sounds.

I have been able, since the communication relating to the sphygmophone was laid before the Royal Society, to make some new observations with it in cases of disease, and to compare

the sounds it causes with the heart-sounds as they are heard through the stethoscope. I have also been able to get the telephonic expression derived from the cardiac pulsations. In a case of regurgitation the sounds on the sphygmophone, taken from the movements of the radial pulse, were very significantly marked. The first sound was short, the second was rather prolonged, the third was divided into two very short and sharp sounds, the last of the two running almost into the first sound without a pause. I suspected, from this, aortic deficiency with mitral obstruction, and the stethoscope confirmed this diagnosis. In the case of a patient who has no physical evidence of structural change of the heart, but who is subject to severe attacks of dyspeptic palpitation, an attack of palpitation came on while she was under examination with the sphygmophone. The sound now produced was of the most singular kind. It resembled precisely the rotatory motion of a large wheel, so that when I shut my eyes I could not believe that I did not hear the motion of a wheel or grinding stone. Looking afterwards at the needle, it was seen to be making the usual movements as far as I could discern, but with great rapidity, yet I could not distinguish the three usual sounds from the telephone, nor aught except the quick rotatory whirr. When I held the telephone close to my ear the whirr was so loud and so perfectly like a wheel it made me feel giddy and nauseated, as if I myself were turning round. The palpitation, which lasted four minutes, stopped suddenly, and after a few hesitant strokes, the usual natural pulse-rhythm was resumed.

When a person who is being examined with the sphygmophone laughs, the pulse beat is soon quickened, and the sounds produced are sometimes almost like an echo of the laugh itself. I notice also that with quick action of the pulse, when the beats are over ninety a minute, there is sometimes, together with the ordinary rhythmic sounds, a singular wave-like continuous murmur resembling that which is heard when a shell is held to the ear. I am not sure as yet as to the cause of this murmur: it may be from friction of the blood on the walls of the artery; it may be from friction of the blood itself; it may be a wave produced by the acts of respiration.

The sphygmophone will, I think, be found very useful for detecting minute and obscure pulsations in different parts of the body. It also indicates very quickly the action of alcohol and other medicinal agents on the pulse.

In applying the sphygmophone to persons who have intermittent pulse, and who are timid it is judicious to reduce the volume of sound, the intervals of intermittency and the renewal of beat being singularly unpleasant phenomena when they are brought out in too pronounced a form. In such cases

the operator may either reduce the battery power, or increase the resistance in some part of the circuit, and so bring the sound down to a whisper, to which he can listen by applying the telephone to the ear as if it were a stethoscope. In the consulting-room this is the best plan to adopt; it answers the purpose, and it does not disturb the mind, and thereby the circulation, of the patient.—*Medical Times and Gazette*, May 31, 1879, p. 585.

DISEASES OF THE ORGANS OF RESPIRATION.

16.—ON THE INFLUENCE OF ACONITE IN CONTROLLING PNEUMONIA.

By Dr. WILLIAM DOBIE.

Case 1.—A. B., aged 42, of very good constitution, but dissipated habits, got out of bed one night, and stood for some time with his bare feet on cold flag-stones. A few hours after this he began to shiver, and feel pain in the right infra-mammary region, accompanied by cough and rusty sputa. I saw him the following morning. His breathing was hurried, pulse quickened, and temperature a little over 101° Fahr., and minute moist crepitation was audible over the low part of the right lung. Tincture of aconite (B. P.) was given in two minim doses every half hour for the first two hours, and after that, in minim doses every four hours. The next day his breathing was easier, pulse slower, and temperature 100°. He had slept well all night; indeed so much so that he said,—“I presume you gave me an opiate.” Aconite reduced to one minim night and morning. On the third day the heat fell to 99°, and from that time he got steadily better; was down stairs in a few days, and at business within a week.

Case 2.—C. D., a railway porter, aged 20. Previous health pretty good, with the exception that about six years ago he had pneumonia, which, he says, was followed by some kind of fever. He was at that time three months off work. He has been fairly well ever since until last month, when, after returning home one day from his work he began to shiver and feel very ill. I did not see him that night, but his companion who slept with him, said his skin was so hot that he could not bear to touch him. I visited the patient the following morning, and found him with a temperature of 103°, quick pulse, and hurried respiration. There was troublesome cough with rusty sputa, and the physical signs of pneumonia were quite distinct. Aconite was given as before, and in twenty-four hours the temperature became normal, and never again, so far as I observed, rose beyond that point. Daily observations were made during the three subsequent days. He has continued well ever since.

Case 3.—E. F., aged 68, in pretty good preservation in spite of the fact that he has had repeated attacks of bronchitis; and two years ago had an apoplectic seizure, followed by paralysis, from which, however, he made an almost complete recovery. He got over this winter and spring pretty well until the beginning of April, when he took cold, for which he prudently stayed within doors a few days. At the end of that time, however, he ventured out one very cold afternoon, and as he was returning home felt chill. During the evening he had a severe rigor, and felt very ill, and shortly after began to complain of pain in his right side, accompanied by cough and difficult breathing. I saw him the next morning about 3 o'clock, a.m. His temperature was then 104° ; and he had coughed up a quantity of rusty sputa. Aconite was prescribed in minim doses every half hour for the first four doses, then in half drop doses every four hours. Five hours after the medicine had been begun he vomited, and had his bowels freely opened; after which he fell asleep and slept quietly until 11 o'clock, when I again saw him. His temperature had fallen four degrees; the pain was nearly gone, and the cough and dyspnoea were much relieved. In the evening the temperature rose again to 101.2° ; but the next day it was normal, and remained so for three successive days. The following week he suffered from bronchitis; but there were no signs nor symptoms of any further pneumonia.

Case 4.—G. H., aged 4, was taken suddenly ill on the evening of the 17th inst. I first saw him at 7 o'clock p.m. He was then in a state of semi-consciousness, with a temperature of 103° , and a pulse of 144. His respirations were 60 per minute. There were cough and symptoms of pain, especially when he was moved. Aconite was given in half minim doses every half hour for the first two hours, at the end of which time I saw him again. His temperature was still 103° , but the respirations had fallen to 48. Aconite was continued in the same dose every four hours during the night. About 5 o'clock a.m., he vomited what looked like white of egg, his nurse said, and which, she added, must have been in his stomach some days. This was followed by what the nurse called convulsions. He then fell asleep, and when I visited him the next forenoon, his temperature was normal and his breathing easy. The temperature rose once to 99° , but after that it remained normal. Observations were made for three successive days, at the end of which period I ceased attendance, as the child was then evidently quite well.

Remarks.—There is, of course, nothing new in this use of aconite. Since Dr. S. Ringer's views were published, the profession has been more or less familiar with the virtues of aconite in controlling simple inflammation. I very much question,

however, if the matter has had that attention paid to it which it deserves. It is one thing to know a truth, and quite another to be impressed by it.

With regard to the first three cases, I think there can be no reasonable doubt that we had to deal with pneumonia. Case 4 is not quite so conclusive. It is open to doubt whether or not the attack was one of simple fever associated with acute gastric disturbance. (Even then it is of some importance to know that we have in aconite an agent which may cut short a febricula in twelve hours.) Without, however, wishing to dogmatise, I think the absence at the onset of any distinctive symptoms of digestive derangement, together with the cough and extremely hurried breathing, are wonderfully suggestive of commencing pneumonia.

The anodyne effects of aconite are very generally known; but I very much doubt if its hypnotic action, which was very noticeable in two of the above cases, has been sufficiently recognised.

All the cases, it will be observed, were seen within twenty-four hours, at the outside, from the beginning of the disease; and it is chiefly then that aconite proves of such signal service. Not that it is altogether useless after the first day. On the contrary, I believe that whenever the pulse and temperature run very high and there is no marked prostration, aconite in carefully regulated doses will often be found useful in moderating vascular excitement, relieving pain, and inducing sleep.

I am sorry to extend these observations so far; but I should like to add that I am not enthusiast enough to believe aconite will cut short every case of pneumonia. On the contrary, I feel bound to state that I have seen many cases where it failed. At the same time, the drug appears to me to be deserving of very much more careful attention than it has yet received.—*Practitioner*, June, 1879, p. 401.

17.—ON THE USE OF ACONITE IN PNEUMONIA.

By Dr. A. C. F. RABAGLIATI, M.A., Honorary Surgeon to the Bradford Infirmary.

The cases of pneumonia cut short by the use of tincture of aconite which Dr. Dobie has lately recorded (see preceding article) appear to me deserving of some remarks, which I shall attempt to keep within as short a compass as possible. I have no doubt all the cases were really pneumonias, and the treatment is such as is not only recommended in some text-books of therapeutics, but I should say is in pretty general use among medical men, though not yet perhaps so much so as it ought to

be. It has been my practice for years to treat incipient pneumonias and also other simple inflammations by aconite, which however I have found it convenient to use of $\frac{1}{10}$ th of the strength of the pharmacopœial tincture, the remaining $\frac{9}{10}$ ths being rectified spirit. I think if Dr. Dobie had used this preparation he would not have induced the vomiting which he describes in Cases 3 and 4, and which is no essential part of the cure.

I should like to say a few words on the *rationale* of the use of aconite, and shall do so in language similar to what I used in addressing to your readers some remarks on the question, "Are there Laws of Therapeutics?" In my view aconite follows the universal law of agents (not merely therapeutical remedies) competent to affect the economy, viz., that it acts by *action* and *reaction*, which are contrary to one another. The action of aconite is the same as that of cold: it depresses the vital power of the sympathetic nerves, and contracts the vaso-motor muscular fibres. The reaction of aconite (or, properly speaking, of the economy affected by aconite) is shown in relaxation of the spasm, and in congestion of the capillary vessels. If one attends only to this latter occurrence, one may say, aconite causes the phenomena of the (simple) feverish state. In point of fact, however, this is only half the truth, which is, stated fully, this: aconite causes first spanæmia, and second, congestion of the capillary vessels. Now what is (simple) inflammation? Is it not the reaction due to exposing the body to cold? And are not the steps of the process first spanæmia of the exposed part, and second congestion of the same? The latter condition we are in the habit of calling inflammation, though only by overlooking the fact that it is a secondary, and not a primary occurrence—an error, by the way, that Cullen did not fall into. Aconite, then, cures inflammation because its action depresses the vaso-motor system, and therefore lowers the excitement of the febrile reaction. For which reason the dose should be small, so small as not to induce any secondary wave of reaction, which might leave the patient as bad as he was before. Being small, the dose should be frequently repeated; and I am in the habit of administering it, not every hour or half-hour, but every ten or fifteen minutes, till lowering of pulse and temperature, moisture of skin, and sleep, are induced. Not once, but many times, have I been able thus to check incipient pneumonia, peritonitis, pleurisy, and tonsillitis; and in children particularly the effects are marvellous. The induction of sleep is difficult to explain. I do not think it is a direct effect of aconite, but rather an indirect one, obtained by combating the excitement, and *allowing* rather than *inducing* nature to complete the cure by rest. Sweating

seems also to be an indirect effect of the administration, to be explained in some similar way.

Next comes the question, Why does aconite act in pneumonia on the capillaries of the lung, in pleurisy of the pleura, in tonsillitis of the tonsil, &c.? Because, it is replied, these parts when inflamed are more susceptible than when they are in their normal state, just as they are more susceptible to painful pressure. Aconite has a wide range of action, but is determined to act on a given part of the capillary system by the susceptibility of that part.

Now, to show further that this view of action and reaction is sound, let me remind you that the *reaction* caused by cold can be itself cured by the *action* of cold. Judicious "packing" will resolve a pneumonia about as well as aconite. Again: excessive pressure will cause inflammation of the skin as its reactionary effect, and I have, on one occasion, which I shall always remember, cured the inflammatory erysipelas which followed excision of the knee-joint in a young woman by a carefully applied bandage. The water used for packing in the former case should not be too cold, neither should the bandage in the latter be too tightly applied; otherwise reaction might in turn be set up and the inflammatory symptoms increased. Supposing the erysipelas in the case just referred to to have been due to over-pressure, these instances of treatment are not covered by the formula *similia similibus*, but must be stated as *eadem iisdem*, τὰ αὐτὰ τοῖς αὐτοῖς, or *identical by identical*. Even then, however, the formula would be but an empirical one, which is explained when action and reaction, its simpler expression, are considered and understood. Neither is it true to say, as some do, "a moderate dose acts as you describe, but a large one has only what you call the secondary action." For, first, what is a large dose, and what a small, and what a moderate one? And second, when the dose is big enough, only the primary action is induced, since the patient does not live to have the secondary; just as a man exposed all night in the snow has neither inflammation nor frost-bite, since he is killed before he has time to have either. Supposing one were to argue from that, that cold has an opposite action in small quantities to what it exerts in large quantities! Thirdly, just as the mass of medical men to-day think of inflammation as a congestion simply overlooking the spanæmia which precedes that condition (and how many of us have given due attention to the subnormal pulse and temperature which succeed the congestion?), so, I am convinced, have many others overlooked the depression induced by aconite previously to the induction of the feverish state, and hence have said, "Aconite induces the feverish state." There is reason for thinking that the

reaction of a remedy or agent is proportional to the action of the same: but I will not weary your readers by repeating at length what has been said on a former occasion. Meantime I think I have shown that—

1. Aconite, like other agents, has on the economy an action and a reaction. The former is a spanæmic, the latter a congestive action.

2. Small and large doses have only an apparent, not a real contrariety of action.

3. Aconite acts in simple inflammations by combating the reaction of cold, which is commonly, but only by oversight, called inflammation.—*Practitioner*, August 1879, p. 87.

18.—ON LOCAL EMPYEMA: ITS DIAGNOSIS AND TREATMENT.

By Dr. SAMUEL WILKS, F.R.S., Physician to Guy's Hospital.

Diseases of the chest, being most rife, are those which of necessity most concern us; and, for my own part, amongst the numberless affections of the lungs and their appendages which, during the last two or three years, have presented the most difficult problems of treatment, have been localised pleuritic effusions, or empyemata. The cases are common enough to have come before all members of the Association; and I judge, therefore, every one must have experienced some of my difficulties. If, however, any one have come here to learn the best method of their treatment, he will be disappointed. I may as well say, I do not come prepared to advance any special plan, but rather hope to learn something from members to help me out of my own difficulties.

As regards the nature of these cases: in a complete effusion of serum, the disease may come on insidiously, and the patient never seek advice until the chest is full. In purulent effusion, the same slow formation may occur, but generally the patient is already ill, and the process going on in the chest is manifest. In localised empyema, however, the history of an acute attack is always to be found. (I shall speak rather dogmatically, so as to save time.) A general acute pleurisy may have occurred, a partial absorption then have taken place, leaving a certain amount of matter locked up at the bottom of the chest.

And now I am going to make a statement, as the result of my experience, and which I particularly want sifted by the meeting. If, under any circumstances whatever of an inflammatory attack in the chest, under whatever name that attack may be called, there result localised dulness, with absence of breath-sound, and perhaps distant tubular breathing, an empyema may be safely suspected. This is an opinion which I have reached from the

very uniform termination of a number of disputed cases, where these physical signs have existed. I mean this: let a patient have an undoubted acute pleurisy, and if, after the urgent symptoms have passed, there still remain the above physical signs in one part of the chest, some fluid may be considered to be left, and this is pus. If the case be one of pleuropneumonia, and there result after the acute attack the same local physical signs, then also an empyema is present; and more than this, if it have been thought from the nature of the attack that the inflammation was confined to the lung alone, and yet, after some time, when all other symptoms have ceased, there remain a localised dulness, with absence of lung breath-sound, it implies that a pleurisy had accompanied or succeeded the pneumonia, and, as a result, a local empyema is present. Thus, under all circumstances, with an history of an inflammatory attack, the result and the conclusion are the same.

This was not always my opinion, nor is it generally held; for it is mainly from contrary diagnoses having been given in several cases, where one uniform condition invariably turned out to be present, that this conclusion was forced upon me. I remember well how two of the physicians of this hospital—Dr. Addison and Dr. Hughes—used to contend over the question of consolidated lung and pleuritic effusion in certain cases; and I myself, for many years, used to teach that a lung which had become hepatised took a long time to recover itself, being led to this view by the continuance of the physical signs above-named, and by following the guidance of the best writers on pneumonia. I taught it, however, against my better judgment, always declaring that I could not imagine what this half-recovered lung was like, or what the results of an acute pneumonia could be, since I had no knowledge of such condition in the *post-mortem* room. I recognised a chronic pneumonia after death; but this, not to mention the fibroid form, was the disease known as acute phthisis—a disease where no doubt the inflammatory process had progressed rapidly and was accompanied by pyrexia, but yet never had the acute nature of ordinary plastic pneumonia. These chronic pneumonias are found accidentally in persons who have been ailing three or four weeks with cold or cough, and who have never taken to their beds; they are acute diseases of the lung undoubtedly, in comparison with ordinary cases of phthisis, but yet never having the acute nature of the ordinary lobar sthenic pneumonia. This is a disease in which the lung becomes consolidated in thirty-six or forty-eight hours, and recovers itself in as quick a time, leaving, I believe, nothing behind. This is positive knowledge gained by inspections; and yet, from physical signs such as I have mentioned, existing after the acute attack was

over, I was fain to admit, as others had done, that weeks might elapse before the lung resumed its healthy state. I hold this opinion no longer, and believe that the slight undetected pleurisy which accompanied the pneumonia continued to progress, and that the physical signs observed were due to fluid compressing the lung, rather than the lung itself being consolidated. Of course, members must remember that I am dealing with only limited portions of the chest, where the diagnostic signs between consolidation and effusion cannot be so marked as where the whole side is involved. In denying that chronic disease is a result of acute, I do not speak of cirrhosis of the lung resulting from pleurisy.

Cases which I might mention to illustrate these facts are numerous; but I will only quote some instances which are fresh in my memory. A young man, after an acute attack in the chest, was sent up to London some weeks afterwards for advice as to the condition of his lungs. The right side was dull below, with loss of respiratory sounds, distant tubular breathing, and lessened vocal fremitus. Lately, he had begun to expectorate muco-purulent matter. One of the best known chest-physicians in London said he had a breaking-up lung. The case went on for some weeks, when death occurred, and a *post-mortem* examination clearly showed that the lung was secondarily involved, the original disease being an empyema. The case was of interest to me, as I suggested thoracentesis in the first instance.

A young girl, after some acute attack of the chest, made a good recovery up to a certain point, when she was taken to a very good special authority for chest-disease. He said the lung had not recovered from its consolidation, and he recommended her to Nice for the winter. While there she began to cough up a quantity of purulent matter, and was brought home. It was then clear that she was evacuating an empyema through the lung. She gradually sank and died.

I am now occasionally seeing a young lady who, whilst at school four years ago, had what was called inflammation of the lungs, and I have no doubt that she had pneumonia. After recovery and returning home, being short-breathed, her chest was examined, and it was found that the lower part posteriorly was dull, with absence of good breath-sounds. Opinions were sought, and they were to the effect that the consolidated lung had not recovered itself. Nearly two years had elapsed, when she began to cough and expectorate; and I was asked to see her, on the supposition that the lung was now breaking up and she was falling into a consumption. Before visiting her, I expressed an opinion as to my view of the case, saying I had never yet seen a case of consumption following an acute plastic

pneumonia. An examination of the chest proved the lung to be sound, but that there was a localised collection of pus at the bottom of the chest. This was emptied, and the expectoration ceased. Subsequently, the matter formed again and was brought up. This has been going on ever since, nearly four years having elapsed since the acute attack.

Some here may, perhaps, remember the case of a man who came into the clinical ward two years ago, under a colleague, with acute pleuropneumonia on both sides. After the acute symptoms had passed, dulness was still found at the posterior part of the chest on both sides, whilst the anterior part of the chest was resonant. He was constantly examined in the hope of finding an alteration in the physical signs, but they still remained the same. He was then transferred to my ward, where it was suggested that he had thickened pleura, but that recovery would eventually occur. He never, however, left his bed; and it was thought by my clerks that the lung was probably being disorganised, and that a phthisical condition was going on. My objection to this was, that I knew nothing of phthisis beginning as an acute pleuropneumonia. He gradually grew more feeble, and died at the end of nine months with universal lardaceous disease; he had a double empyema, confined to the posterior part of the chest, and compressed lung.

What is the natural termination of these cases? The one is the drying up of the abscess, and the frequency of this occurrence is one which ought to be ascertained. I myself have not often met with them on the *post-mortem* table; but I think I have heard my colleague (Dr. Goodhart) express a contrary opinion. Then, again, their direct drainage through the lung, and this is common enough; but I am not sure that the mode is always the same. It must be well known that, whilst pleuritic abscess is discharging itself, there are frequently no physical signs of a fistulous opening in the lung; and, in a *post-mortem* examination of such cases, we may seek in vain for a direct communication between the lung and the pleuritic cavity—at all events, there is no pneumothorax. So impressed was the late Dr. Barlow with this fact, that he surmised that the removal of purulent matter was not direct, but was formed in the bronchial tubes, and that the pleuritic abscess was only the pabulum for its growth. Another reason he gave in favour of this view was that patients with empyema sometimes were seized with a diarrhoea, and then got rid of their thoracic effusion. I might mention, in connection with this view, that the young lady whose case I have watched for four years has had several attacks of severe illness, commencing with rigors, passing on towards collapse, and ending with diarrhoea. We suspect that these attacks are pyæmic, or rather that some

absorption or introduction of purulent matter takes place into the blood, causing the symptoms I name.

These cases, then, may end favourably by simply drying up or by discharging themselves through the lungs. It might be thought, therefore, that the better course was to leave them alone. This has hitherto been the practice, partly wittingly, but more often from an ignorance of the nature of the case. Having, however, seen in about a year five such cases terminate fatally, I hesitate to think that leaving them to nature is altogether the best plan.

The question of treatment is different from that which is applicable to a general pleuritic effusion. The indications for removal in this case are clear: the danger to life from pressure on neighbouring organs, or from the constitutional disturbance connected with a chest full of pus. In localised empyema, there may be no symptoms whatever; but eventually danger may accrue from the abscess involving the lung. What we want to know is the extent of this risk—how many cases are fatal in this way, compared with those which are evacuated successfully through the lung, and with those which simply dry up? I say nothing of a natural evacuation externally; for this I have seldom seen. Had time allowed, this might have been a point for discussion. I know such cases are spoken of; but it is difficult to understand why they should occur, seeing that the lung would much more readily give way by pressure than the chest-walls. I do not seem to remember ever seeing an empyema make its way externally until the lung had first become compressed. To return to the question of risks of allowing a localised empyema to remain, I consider that they are great, having seen of late so many fatal cases. If this be so, the better method is to tap them. In the first instance, an exploratory needle may be used, and then a larger trocar, followed by a drainage-tube, or closing the opening. I think, in the first instance, I should be inclined to close the opening, as a partial evacuation might cause the absorption or drying up of the rest, as is sometimes seen.

The question is: given an abscess, shall we, as the surgeon recommends in ordinary abscess, evacuate it externally, fearing it may take a dangerous course internally? Under both circumstances, it must take time for the cavity to close, seeing that on one side is the bony wall of the chest, and on the other indurated tissue formed by pleura and adjacent lung. The diagnosis of the abscess I put on the simple grounds already mentioned: a localised dulness and absence of breath-sounds, remaining after any inflammatory attack of the chest.—*British Medical Journal*, June 12, 1879, p. 928.

19.—ON THE USE OF ATROPIA IN THE TREATMENT OF THE NIGHT-SWEATS OF PHTHISIS.

By Dr. WILLIAM MURRELL, Lecturer on Physiology at the Westminster Hospital, Assistant Physician to the Royal Hospital for Diseases of the Chest.

Dr. Milner Fothergill in an interesting article on Anhidrotics (*Retrospect*, vol. 75, p. 43,) says:—"The most potent of all anhidrotics in my experience is unquestionably belladonna. We are indebted to Dr. Sydney Ringer for our knowledge of this property of belladonna; and the debt we owe to him can only be sufficiently estimated by those who have had an extensive experience of phthisis, and who give the drug a fair trial. I have no hesitation in saying that the use of this agent completely changes the aspect of many cases of pulmonary phthisis. For the arrest of the exhausting night-perspirations of phthisis belladonna is as potent as digitalis is in giving tone to a feeble heart." Dr. Ringer was led to try the influence of belladonna on sweating from the remarkable power it exhibits of checking the secretion of milk when applied to the breast. Soon after the publication of his papers, I made at his suggestion some observations with the view of testing the value of hypodermic injections of small quantities of atropia in checking the sweating of phthisis. The drug employed was the sulphate—the dose from $\frac{1}{200}$ th to $\frac{1}{100}$ th grain. The conclusions were arrived at as the result of experiments made on sixty patients, who were seen at least twice a day, morning and evening.

Age, sex, and temperament in no way influenced the results obtained; the injections were successful in men and women, in young and old.

The presence or absence of fever did not influence the result. In nearly all the cases there was some elevation of temperament; in some it was but little above the normal, whilst in others it ranged from 102° to 103° F., or even higher.

The beneficial effects of the treatment are not confined to any particular stage of the disease.

The presence or absence of debility does not affect the result; in some cases the patients were in bed, suffering from great prostration, whilst in others they were well enough to be out of doors the greater part of the day.

The fact of the perspiration having or not having commenced at the time of the injection is of no importance. In a case in which the patient was perspiring very profusely over the whole body an injection was given,—in five minutes the perspiration was very much less, and at the end of half an hour his skin was quite dry.

The benefit derived from the injection lasts in most cases for several nights, so that it need not be repeated every day. An

injection once a week or once in ten days will often suffice to keep the perspiration in check.

In many cases the effect of the drug is delayed, no benefit being experienced on the first night, but on the second and succeeding nights the sweating is completely checked. The beneficial effects of the drug, when lasting several nights, appear to pass off gradually, the perspiration coming on earlier and earlier every night. Thus it was noticed that gr. $\frac{1}{100}$ given at bed-time would often produce no effect that night; on the next night—no further injection being given—the perspiration would be completely stopped; on the third night the patient would be free from perspiration till five or six in the morning; on the fourth night it would begin at two or three a.m.; whilst on the fifth and subsequent nights it would be as bad as ever.

It is not essential to give the injection at bed-time; in fact, in some cases when the action of the drug is required on any particular night, the earlier in the day it is given the more likely it is to prove successful. Sometimes, too, when the drug is given at bed-time, partial relief is obtained on that night, whilst on the second and subsequent nights the full effects of the drug are experienced.

The injection of atropia has been used with success where oxide of zinc, gallic acid, and other drugs have been tried in vain. The gr. $\frac{1}{100}$ will often succeed where gr. $\frac{1}{200}$ has failed. One injection may completely stop the perspiration, and although the patient remains under observation for some weeks, there is no complaint of its return. These cases are not common, but it not unfrequently happens that after a few injections the perspiration, although not completely stopped, is checked to such an extent as to render further treatment unnecessary.

An injection of atropia will often relieve cough when in excess of the amount of expectoration, and thus enable the patient to obtain a good night's rest; but it is not only in this way that the perspiration is checked, for night-sweats are benefited when there is not much cough, and the patient sleeps well.

Patients rarely complain of any unpleasant symptom even when the larger dose is given. Dryness of the throat is a condition so common in sufferers from phthisis that any increase in this symptom, unless very marked, will pass almost unnoticed.

The number of cases in which marked and permanent benefit is not derived even from these small doses does not amount to more than eight or ten per cent. The observations having been made for experimental purposes, the drug was given hypodermically, but in practice it would be found more convenient to give it by mouth, in a proportionately larger dose. It may be given in pill or in solution, or in granules. It is stated by Dr. Aquilla Smith that a solution of sulphate of atropia in

camphor water (made with distilled water) will not spoil by long keeping. As to the dose, [Dr. Fothergill usually commences with gr. $\frac{1}{75}$ by mouth, and increases it to gr. $\frac{1}{25}$. Speaking from a large experience of the drug, he finds that it may be freely used without apprehension as to any serious toxic effects appearing. "Even with gr. $\frac{1}{25}$ of atropine the patients," he says, "do not complain much: some dryness of the throat and a little indistinctness of vision being all; while all prefer these to their dreaded sweats. These effects wear off in a day or two after the drug is discontinued, or even the dose reduced. I have not yet seen any alarming symptoms produced. This I attribute to the gradual increase of the dose; and I have but little doubt that if gr. $\frac{1}{25}$ were given at first, many cases would show marked toxic symptoms." Dr. J. M. Williamson mentions a case in which the eightieth of a grain given by mouth produced severe symptoms of poisoning. M. Vulpian employs granules each containing half a milligramme (about gr. $\frac{1}{130}$.)

Atropia will stop other forms of sweating, such as the sweating of acute rheumatism, prolonged suppuration, convalescence, &c.

Atropia and belladonna check sweating by a peripheric action on the sweat glands, but it is not unlikely that they have also a direct central action.—*Practitioner*, August 1879, p. 93.

20.—THE CLIMATE OF DAVOS-PLATZ IN THE TREATMENT OF PHTHISIS.

By Dr. T. CLIFFORD ALLBUTT, Physician to the General Infirmary at Leeds.

[Dr. Clifford Allbutt gives a list of 85 cases which have been treated more or less at Davos, and concludes his interesting series with the following remarks.]

It will be seen that the cases are wholly unselected, and extracted as they came from the complete notebooks submitted to me. Of some of the cases I have personal knowledge. Considering the drawbacks of the past season these results are surely very striking. Omitting the cases published last June, we have before us now notes of 66 cases, 55 of which were phthisis in one form or other—55 cases of an organic disease which is the despair of the physician and the terror of the public. Now, of these 55 cases, which presented themselves in all stages, and many in their last hope, no less than 37 did really well. Some recovered entirely, others made great progress, and are likely to recover in another season. Some of these cases, such as Nos. 33, 41, and 83, are most remarkable, and Case 66 is a good instance of the wonderful effects of Davos upon many cases, even at the outset of treatment. Of those that died I am

enabled to say, from personal knowledge, that more than one of these felt so much better at Davos than in England as to decline or be unwilling to return home, and to contradict the opinion that they ought never to have been sent thither.

I have taken care to assure myself that the physical signs have been accurately noted, and I may add that in nearly all cases the examinations of the chests were made upon patients in bed in the early morning, before movement had aided in the expulsion of phlegm. A few cases of phthisis were syphilitic, but for obvious reasons these have been kept back, and it may be said in general terms that such cases derive far less benefit at Davos than at places where the special malady can be more thoroughly combated. It is true of *poitrinaires* at Davos, as at every other health-resort, that infirmity of purpose, conflicting engagements, narrowness of means, and the like, often interfere with the regularity or the completion of the curative course. It is well that all patients intending to visit Davos should be made aware that although, on account of a certain exhilaration in the air, the monotony of Davos is often unfelt, yet it is extremely monotonous, and not well provided with comforts and attractions either within or outside the hotels. The chief of these defects are in course of removal, and I am glad to hear that English nurses are now going out for the purpose of attending upon the invalided. Davos, however, is unsuited for the prostrate invalid. It has been improperly said that patients go thither expecting to breathe the mountain air, but that actually they breathe the air of stoves. The very reverse is the case. That heating by porcelain stoves is almost universal abroad is true enough, and such stoves give an equable warmth by no means objectionable if ventilation can be had with them. Now it is usual for patients soon to become so inured to the mountain air as to sleep with open windows; while, during the day, to spend as much time as possible in the open air is of the essence of the treatment. In fine seasons a great part of the patient's time can thus be spent day by day. Unfortunately the above notes testify to the frequency of colds, due to the unusual damp and the thaws, which, this winter, thus interfered with recovery, and kept the patients more than usual in-doors. Young men, and old ones too for that matter, will forget their instructions and spend their days at drinking-bars or billiard-rooms, and then go away complaining that they are no better. Others fall into the contrary error; they get rapidly better, seek no medical advice, or pooh-pooh it, career over the hill, wet their feet, wear foolish clothes, and so forth; and yet, after all, these patients as a rule do these things with a great measure of impunity. The striking and rapid improvement is generally in patients who are able to walk. The use of strong douches, so great a part

of the early therapeutics of Davos, has now given way almost to a mild tubbing; but alcohol is still found the great aid in combating evening fever.

Now, what practical help is the practitioner to draw from these records? That the results in this stern still climate are better than those obtained in the lovely, capricious, and treacherous Riviera is, I think, evident enough. Whether they compare to any great advantage with the curative effects of sea voyages is hard to say, but a residence at Davos, monotonous as it may be, yet in its summer and winter pleasures, in its greater and increasing comforts, in the systematic medical supervision, and in its open communications with home, must be far more desirable than the ordinary sea voyage.

The next question the physician will ask himself is, What class of cases are likely to do well at Davos, and in what stage? the latter question depending on the former so far as this, that a case of a proper kind might hopefully be sent in a later stage than a case of a less appropriate kind. Speaking generally, the cases which do so well at Davos, and of course elsewhere also, are cases of acquired phthisis when the patient is yet young and in possession of a good frame and fair physical powers. Such a case, sent, say, with consolidation of one apex or a simple cavity, is tolerably sure of cure. And I believe such cases may be sent to Davos not without hope even in advanced stages. Passing over the many intermediate cases and turning to those with strong hereditary tendency, with red tongues and irritable stomachs, with diarrhoea at times, rapid emaciation, excited circulation, and sharp evening fevers, and perhaps with neurotic complications, of these I have less hope even when the pulmonary signs are limited. I think such cases should scarcely be sent at all when the signs in the chest are much advanced, or the presence of abundant tubercle suspected. To send prostrate emaciated patients of this class "to breathe stove air at Davos," as Dr. Bennet says, is quite unjustifiable; and what are we to say of those who send patients with diffused tubercle, and suffering perhaps in lung, larynx, and bowel? Let such false kindness be resisted; let the physician learn to speak the grave truth to such sufferers, and tell them how little they have to hope for from anything but the comforts of home. Laryngeal phthisis, I fancy, does not do well at Davos, even when the patient is otherwise in fair condition, but on this I do not wish to dogmatise. To pass from phthisis to other chest diseases, it should be widely known that all heart diseases do better elsewhere, and that bronchial conditions do badly if associated with defective cardiac action or with senility. Asthmatic persons are so capricious that each patient has to find out a haven for himself; a given asthmatic may lose his asthma at Davos.

Only young persons should try the experiment, and persons free from emphysema. Dyspeptic patients, again, may or may not do well at Davos, and should not go thither without a previous careful diagnosis of their case. Nearly all nervous complaints will do as well or better at other places. Finally, I find it necessary to impress strongly upon all lay persons the folly of going to Davos for health purposes independently of medical advice. I have some diffidence in saying this because I may seem to say it in the interest of my medical brethren. I say it, however, with a single eye to the good of patients themselves, who are often foolish enough to risk at least the loss of time and money, if nothing more, by seeking a health resort unsuited to the kind or degree of their malady.—*Lancet*, July 26, 1879, p. 118.

21.—REMARKS ON THE CLIMATE OF DAVOS-PLATZ.

By Dr. ARTHUR HILL HASSALL, Physician to the National Hospital for Consumption and Diseases of the Chest.

There is a fashion about health resorts as about most other things. For a time one particular place is recommended; after a while the reputation of this begins to wane, and another comes in favour. This remark applies especially to Davos-Platz, which it is now the fashion to recommend. Unfortunately but too often the recommendation of any particular place is, in the first instance, based upon an imperfect knowledge of the facts, and it is only when these have become fully ascertained that a reaction occurs, such as, I believe, will ensue in the case of Davos-Platz.

Some years since, having made a fair recovery from a pneumonic attack, I visited the Upper Engadine, the valley of the Davos, and the lower Engadine. Davos, even at that time, had acquired some degree of reputation, principally among Germans, as a health resort, and the impressions which I then derived, and which have been more recently confirmed by the observations of others, have induced me to write the present article.

I left London in the beginning of August, the weather being hot and sultry. On nearing the Upper Engadine two or three days afterwards, snow was falling fast, and the diligence entered Samaden covered with it; in the brief space of a few hours one had passed from summer to winter. I found the hotel full, and had to sleep in a large bedroom over a cow-house, without covering to the floor and scarcely any to the bed. The days were very short; it was intensely hot in the sunshine, which, however, lasted a few hours only, and equally cold out of it. At this elevation (over 6000 feet) my circulation and respiration became hurried, my breathing so short that I was

filled with apprehension, and altogether I felt so depressed, nervous, and uncomfortable, that it was only by the exercise of great self-control that I was able to remain in the Upper Engadine for a few days. On making any slight exertion or attempting any ascent, I became exceedingly breathless. What with this difficulty of breathing, being scorched in the sun, chilled in the shade, and still more chilled at night, I was glad enough to escape from the Upper Engadine, notwithstanding all its glorious mountain and lake scenery. I afterwards repaired to Davos-Platz, which has an elevation above the sea of 5200 feet, being about 1000 feet lower than the Upper Engadine. Here I experienced, although to a somewhat less extent, the same difficulty of breathing, the same apprehension and nervousness. I made many enquiries as to the winter experiences at Davos, and I came away by no means favourably impressed. I next visited the lower Engadine, about another 1000 feet further down, and it was here only that I began to breathe with comfort and that my distressing sensations left me. Now, it appears that most persons who go to Davos with extensive lung affection and with diminished capacity of respiration, experience there the same acceleration of respiration and circulation, the same breathlessness and nervous sensations. These conditions are due to lessened atmospheric pressure and rarefaction of the air, combined with diminished lung capacity.

The valley of Davos is rather short, and may be described as running east and west, with a certain amount of mountain protection at either end; but it is open to some extent to the easterly and westerly winds. The meteorology of Davos has only as yet been recorded for short periods; but enough is known to determine pretty clearly the general characteristics of the climate. Some valuable data for the months of November 1878 and January 1879 appeared some time back in the *Lancet*; those for the latter month, usually the coldest in the year, being particularly interesting and complete. From these it appears that the barometer ranged between 24.35 and 25.15 inches, showing a great diminution in the atmospheric pressure; that the maximum shade temperature showed a range varying between 15.8° and 47.5° Fahrenheit, the temperature being below freezing point on no less than seventeen out of the thirty-one days in the month. The minimum ranged between 7½° below zero and 35.0° above, it being above the freezing point on one occasion only during the month. The solar or radiation thermometer showed a variation ranging between 34.0° and 141.0°, a difference of no less than 107°. The dry and wet bulb thermometers showed a difference of 2.80° only, indicating remarkable dryness of the air, although in one day there was a difference between the two bulbs of no less than 17°. The

column, however, of the relative humidity of the air, 100 marking the point of saturation, appears to show that the atmosphere is really less dry than a comparison of the dry and wet bulbs would lead one to suppose. On only eleven days in the month was the humidity less than 50, while on more than that number of days it either approximated, or even reached the point of saturation. Southerly winds alternated with northerly in about equal proportion; there were but five complete days of bright sunshine in the month, nine on which snow fell, fifteen more or less overcast, and one on which there was mist. The observations published for November 1878 are corroborative in the main of those above given, although that month on the whole was by no means so cold as January. It thus appears that the range of temperature experienced at Davos during the month of January 1879 was something enormous, it varying from 7.5° below zero up to 141.0° ; the radiation thermometer even, as already remarked, showing a difference of 107° . It is difficult to comprehend how such extreme variations of temperature can be otherwise than injurious in the great majority of detected cases of phthisis, which are mostly cases in an advanced stage of the disease.

The valley of Davos, lying east and west, and being tolerably broad, no doubt receives a considerable amount of sunshine; but the days in winter are short; and it appears, by the statistics quoted, that there were only five days of bright sunshine in the month, although the sun shone on other days, more or less obscured by clouds, the passage of each cloud over the sun's disc being attended, of course, with a great and immediate change of temperature.

The journey to Davos is by no means an easy one; that portion of it from Landquart, occupying eight hours, is accomplished as far as Klosters in a diligence, and from thence in a sleigh, which, unless otherwise ordered, will be an open one. Patients are recommended to reach Davos in the beginning of October, in order that they may become in a measure acclimatised before the great fall of snow which usually occurs in the middle of November—when, in fact, the place becomes snowed up and pretty well cut off from the rest of the world—not a green twig, flower, or bird being visible for months. During the winter fresh falls of snow frequently take place, that which has already fallen becoming consolidated, and at times partially melted. Usually, however, no great thaw occurs until near the end of March; but this is not always the case. Sometimes there are considerable thaws, and then the air becomes indescribably chilly and damp, colds are rapidly caught, and many persons are laid up. We all know what a thaw means in England, where the snow seldom has a depth of more than a

few inches ; how much worse, then, must it be in Davos, where the snow is several feet deep. It is on this account that patients are recommended to leave Davos not later than the beginning of April. But where can they go to at this inclement season ? It is far too early to return home. A warm place, even were one readily accessible, but which is not to be found nearer than the south of France or the Riviera, would scarcely be suitable after the Siberian winter of Davos. Indeed there is reason to believe it will eventually appear that one of the great disadvantages of Davos is, that a winter passed there renders the frame subsequently unfitted for warmer and more temperate climates, and that, quitting its bracing and freezing atmosphere, there is great risk of the speedy loss of whatever increase of strength and health may have been derived from a winter there.

The food at Davos is very fair, suitable for those who have good appetites and are able to take daily exercise, but, as might be supposed, there is a great deficiency of fresh vegetables. Stimulants are freely indulged in, and are sometimes taken to an injurious extent. When once a patient is laid up, is confined to his room, and loses his appetite, his position becomes anything but desirable ; the little luxuries and delicacies then so necessary are scarcely obtainable.

Furthermore, but little reliance is placed on medicines, which are seldom administered by the foreign medical men practising there, even in cases which would appear to us to urgently call for them, and there is no English medical man at Davos. It is boasted that the air alone is the curative agent. Let us see how far this view is sustained by the facts. Few invalids who go to Davos spend more on the average in the open air than some four or five hours out of the twenty-four which go to make up the day and night. Taking the average at five hours, nineteen are spent in the hotel. The external air respired, owing to diminished pressure, expands and occupies much more space. Again, when the sun shines, this rarefaction is still further increased, the capacity of the lungs remaining the same, and consequently at each respiration less oxygen is taken into the system than when the air possesses its usual degree of condensation. The patient breathes what may be termed a diluted or thin air ; in his rooms at the hotel he also breathes a rarefied air, but now warmed by stoves, and rendered impure by the number of persons congregated together in a small space, all breathing the same air, and also by the fact that the closely-fitting and double doors and windows, rendered necessary to keep out the cold, prevent that motion and renewal of the air which is so essential under such circumstances. If any aid to this air is required, it is sought for not so much in medicines as in hydropathy, which we have heard, in some chest cases, is

carried out, with a view to reduce fever, to the extent of what is called the "snow bath." Hydropathic remedies and appliances in the majority of lung diseases, and particularly in phthisis, are as a rule strongly and rightly condemned by English practitioners. I have said that stimulants are sometimes freely indulged in, and it appears that, occasionally at least, large quantities of brandy are ordered with a view to check fever. One gentleman recently informed me that he had taken under orders a pint of brandy daily for five weeks, and, finding that it destroyed his appetite and did not produce the desired result as respects the fever, he refused to continue it, and took on his own responsibility large doses of quinine with much benefit.

That the sharp, keen, cold atmosphere of Davos should brace up the muscular system, increase the appetite, and promote digestion, is nothing more than might have been anticipated from the known effects of cold in other localities; where these results ensue, and they do so chiefly in cases where there is strength and vigour enough to allow of almost daily exercise being taken, the patient is, doubtless, placed in a very favourable position for gaining strength, and with it health. And in the remarks which I have made I by no means desire to make it appear that Davos is not a suitable winter station for certain classes of invalids; but I wish particularly to show the necessity for the exercise of extreme caution in the selection of cases of lung affection which are allowed to proceed there. The fact that some persons suffering from phthisis go to Davos, gain in weight, and improve in their condition, affords no proof whatever that Davos is the best place to which such cases can be sent. A very large proportion of cases derive benefit, no matter to which of our health resorts they go.—*Lancet*, August 2, 1879, p. 152.

22.—ON THE THERAPEUTICAL VALUE OF DRUG-SMOKING, AND OF FUMIGATION IN ASTHMA.

By Dr. REGINALD E. THOMPSON.

It is a matter of some difficulty to analyse with any certainty those mixtures of vegetable and other substances which are sold as remedies for asthma, but an examination of them shows conclusively that they are composite, different leaves being found on submitting any sample to microscopical analysis; some of them contain opium, others do not; most of them contain lobelia, and it may be stated with some certainty that all of them have stramonium for their basis.

If such remedies are tried in a number of cases it is surprising how one remedy at one time appears to be of the greatest

service, another at another time: much depends, doubtless, upon the method of preparation and preservation, and upon the care with which the drugs are selected, and the best secret remedy I know (Himrod's) is evidently well prepared and preserved, the leaves of which it is composed being fresh and green.

Remedies for asthma are supplied in three forms: a powder which is burnt, and *the fumes of which are inhaled*; and cigarettes, composed of tobacco combined with various drugs; or of paper dipped in a solution of the drugs, *to be smoked*.

The best method of preparing drugs for the powder form of remedy appears to me to be this: the leaves of the vegetables used should be procured in good condition and perfectly fresh; they should then be soaked in a solution of nitre (25 per cent.) and the leaves then dried by gentle heat and powdered. I have made use of the various neurotics in this manner in asthma, first separately, in order to ascertain the individual value of the remedy, and then in combination, and the experiments have now been carried on for many months, and I am disposed to place them in the following order of merit:—1, Opium; 2, Stramonium; 3, Cannabis indica; 4, Conium; 5, Lobelia.

The three first on the list appear to be the most potent by fumigation, but when administered in the wet method (if I may use the term) cannabis indica is so uncertain and so apt to produce delirium, especially in women, that I prefer conium, a drug from which I have obtained extremely good results when administered by the mouth.

With belladonna I could not satisfy myself that any good results were to be obtained by fumigation, and I consider it far inferior to those given above in whatever way it is administered.

The powder may be used by those patients who are not accustomed or object to smoking cigarettes, or it may be added to the tobacco of those who prefer the use of the pipe.

As regards the composition of the powder, I have had good results from gr. ix. of stramonium and gr. i. of cannabis indica, this being a quantity, which will cover a shilling, sufficient for one fumigation.

But if the patient does not object to smoking I much prefer to administer the remedies in the form described in my previous paper (*Retrospect*, vol. 79, p. 354), namely, paper cigarettes impregnated with tinctures so that the dose may be accurately apportioned.

It will be understood that in suggesting remedies which serve to alleviate the spasmodic dyspnoea of asthma I do not consider that they constitute a mode of treatment calculated to improve the general condition of the patient, or that they are more than

palliatives of an urgent symptom: constitutional treatment by ferruginous tonics and cod-liver oil, or it may be by iodide of potassium or arsenic, must be resorted to, if it be intended to give the asthmatic patient permanent relief from distressing disease. With acute conditions of the disease, with bronchial complications of such a nature as to contra-indicate the use of iron, there is probably no treatment better for a majority of cases than the use of iodide of potassium with stramonium; in many cases of like character I have derived very good results from the administration of hemlock in combination with the hypophosphite of soda, but for the prevention of the disease I know no treatment to compare with iron and cod-liver oil.

But for soothing and diminishing the dyspnoea, neurotics may be used with great effect; and the following combination is that which, up to this time, has given me the best results.

The same form of cigarette is used as described in my former contribution on this subject, and the paper is soaked in the following drugs according to the recipe here given:—*Rx*. Extract of opium, gr. $\frac{1}{64}$; extract of stramonium, gr. $\frac{1}{32}$; tincture of Indian hemp, *M* $\frac{1}{2}$; tincture of hemlock, *M* $1\frac{3}{4}$; tincture of lobelia, *M* $1\frac{3}{4}$; tincture of tobacco, *M* 9; oil of anise, *M* $\frac{1}{8}$; nitre, gr. $\frac{1}{4}$.

Or for a sheet of Swedish paper sufficient to make sixty-four cigarettes the formula may be given thus:—*Rx*. Tincturæ tabaci, 3 x; tincturæ conii, 3 ij; tincturæ lobeliæ, 3 ij; tincturæ cannabis Ind., *M* xxxij; extract. opii, gr. i.; extract. stramonii, gr. ij; olei anisi, *M* viij; potassæ nitratis, gr. xvi; spirit. v. r. ad $\frac{2}{3}$ ii. ss.

This formula, which is a complex one, has only been obtained from repeated experiments, leading step by step to the addition of some effective remedy, and to the elimination of less effectual drugs.

As it is sometimes desirable not to give opium or Indian hemp, I have had cigarettes made with stramonium and lobelia only; so that altogether I have three different kinds for use: opium cigarettes containing a small quantity of opium and stramonium; a compound opiated cigarette containing the drugs given in the formula above, and a stramonium cigarette without opium.

So many correspondents have written to me for samples of the cigarettes, that I have asked Mr. Rowe (of the firm of Messrs. Strickland and Rowe, Cromwell Place, South Kensington) to make them, and he can supply them. I shall be very glad of any suggestion that will help to make them more serviceable for the relief of asthma.—*Practitioner*, August, 1879, p. 100.

23.—ON THE DIAGNOSTIC VALUE OF ALBUMINURIA.

By Dr. ROBERT SAUNDBY, Assistant-Physician to the General Hospital, Birmingham.

The presence of albumen in the urine is generally regarded as indicative of renal disease, especially when it is persistent and is not the accompaniment of more prominent pathological conditions, such as fever, severe inflammatory diseases, or affections of the circulatory system. Albuminuria dependent on local affections of the excretory urinary organs is, of course, excluded.

I have examined the urine of one hundred and forty-five male patients taken *seriatim* as they presented themselves before me in the out-patient department, and of this number no fewer than one hundred and five contained albumen in more or less quantity, easily demonstrated by boiling and adding a drop or two of acetic acid.

A table which I have prepared shows the ages of the patients and the pathological conditions which determined or coincided with albuminous or non-albuminous urine. When I could, I have referred the albuminuria to renal disease. Sixty-six of the cases are tabulated as granular kidney, and one as fatty kidney, or chronic parenchymatous nephritis. The number of cases of granular kidney seems a large proportion: but their symptoms were carefully investigated before a diagnosis was made, and the proportion (sixty-six to a total of three hundred and fifty men, women, and children) does not much exceed the *post-mortem* room statistics of the General Hospital. All who have had pathological experience in this town admit the extreme frequency of this form of kidney-disease.

Under the head Debility, I have classed cases complaining of weakness, in whom no definite pathological changes could be discovered. I may say the same of Dyspepsia; and with these I would group two cases of acne, making in all thirty-two cases, of which seventeen had albumen in their urine. One of these cases of debility was after typhoid fever: a condition in which it sometimes happens that actual renal degeneration occurs, although Bartels scarcely admits it. Within the last six months, I have seen three cases of albuminuria after typhoid fever. In one of these, the urine presented all the characters of parenchymatous nephritis, and there was dropsy; in the others, the symptoms were those of debility with albuminuria. Some of the cases of debility resemble clinically the type described by Dr. Moxon (Guy's Hospital Reports, vol. xxiii, "The Albuminuria of Adolescents"); while others, in their age and general

symptoms, are more closely allied to the class of cases described by Dr. Clement Dukes (British Medical Journal, Nov. 30, 1878, "The Albuminuria of Puberty"); but in others there was nothing characteristic, and to all appearance they were and are healthy young men who were suffering from some very temporary derangement.

Since the observations of Professor Leube of Erlangen, we must believe that transient albuminuria may occur in perfectly healthy persons; and Dr. Moxon and Dr. Dukes have told us how persistent it may be under the conditions they have described. I have tested these urines on each appearance of the patients, and have proved its persistence for weeks and months. The microscopical examination has revealed usually nothing. In one or two cases, there were a few oxalates; and in one or two a few hyaline casts; but the latter especially were quite exceptional, and, as we know, they are of no pathological significance. As the urine examined was always the after-breakfast urine, passed in my presence, I thought it desirable to investigate the conditions more closely; and by the kindness of Dr. Wade and Dr. Foster, I admitted two youths aged respectively 15 and 17, and had them under observation for some weeks. The urine was examined as passed after breakfast, after dinner, after tea, and during the night, and under varying conditions of diet, absolute milk diet and complete rest in bed being enforced for some days. The result was to prove that the albuminuria did not depend solely upon diet or exercise; for, though complete rest and milk-diet diminished the quantity, they did not cause it to disappear, and on leaving the hospital the state of the urine was just the same as on admission. As I have had it suggested to me by more than one friend that spermatorrhœa might account for this symptom, I must be allowed to point out that the seminal and prostatic fluids do not contain any albuminous body coagulable by heat (Simon), and that the sole ground for believing in this as a source of fallacy is an exceptional case observed by Dr. Bence Jones, and specially recorded as exceptional; while in none of my cases has there been any flocculent deposit like semen, or any spermatozoa found in those examined with the microscope. It is necessary to add that gonorrhœa and other urethral discharges have been carefully inquired for, and their absence ascertained.

Albuminuria in phthisis is well enough known; but it must not be supposed that even under these conditions it indicates amyloid or fatty degeneration of the kidneys. Fürbringer has shown that it may sometimes occur independently of all structural lesion, and frequently when the kidneys *post-mortem* show the appearances of chronic venous hyperæmia (cyanotic induration).

Albuminuria in epilepsy was first described by Max Huppert ; and more recently Otto found albumen in the urine twenty-two times in thirty-one cases after epileptic attacks. Both these authors seem to regard it as the immediate and temporary consequence of the vascular disturbance caused by the fit ; but I have notes of several cases in which no fit had occurred for days, yet albumen was present in the urine.

I have frequently observed albumen in the urine of syphilitic patients without being able to suppose that any renal disease existed. There had been no suppurations ; and I cannot believe that amyloid degeneration could be present, because amyloid disease is remarkably rare in this district, while syphilis with albuminuria is not, and because several of the cases were in the earlier stages of the secondary manifestations.

The occurrence of albuminuria in bronchitis calls for no remark, except that a large number of cases of bronchitis have been included under granular kidney, it being so frequent a coincident or consequence of the latter disease.

Albuminuria in connection with tonsillitis has been noticed before, but I cannot say by whom. In one of the cases, the condition was chronic, and might be classed with those considered under the head of debility.

I do not imagine that the albuminuria of chorea is worthy of being considered anything special. The patients in whom I have seen it have all been young boys, in whom, as we have seen, albuminuria is very common.

The case of laryngitis was simply one of catarrhal sore-throat with aphonia, and made a rapid recovery.

The table and the list of diseases I have been considering is not intended to include all the conditions under which albumen may be found in the urine. It is simply a tabulated statement of the result of the examination of the urine of all the male patients who presented themselves to me from the 25th of February to the 5th of April ; and my object is to show, not that albuminuria is a *symptom* of many diseases, but that it is a phenomenon which may be present under such a variety of conditions that *per se* it affords us no indication of renal disease.—*British Medical Journal*, May 10, 1879, p. 699.

SURGERY.

FRACTURES, DISLOCATIONS, AND DISEASES OF THE
BONES, JOINTS, ETC.

24.—A DEMONSTRATION IN ANTISEPTIC SURGERY.

By JOSEPH LISTER, Esq., F.R.S., LL.D., M.D. (Honoris
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King's College, London.

Putrefaction in wounds and abscesses is analogous to the fermentation of a saccharine solution under the influence of the yeast plant—that it is due to the existence and development of minute organisms. To endeavour to prevent the putrefaction of blood or serum in a wound used to be a matter of an absolutely hopeless character as long as the view entertained as to the nature of putrefaction was that of Gay Lussac, who believed that the oxygen of the air was its cause. If a discharge came out—as it must do, for we cannot seal up wounds hermetically, except very small ones—oxygen could get in; and therefore it was, according to that view, absolutely hopeless to try to prevent putrefaction. But when it came to be established, as Pasteur seemed to me to have done, that putrefaction was due to the development of organisms, the matter assumed a totally different form. The question was, not to exclude air, but to dress the wound with an agent capable of destroying these organisms without acting too injuriously upon the wound. We know, for instance, that if lice infest a person's head we can destroy them without injuring the scalp. If itch exist in the skin we may destroy the offending insect without hurting the integument. And thus it seemed not hopeless that we might apply to wounds some agent that should destroy these septic organisms without acting seriously upon the tissues.

The first agent I used for the purpose happens to be that which hitherto has been the most convenient. I am not bound to any particular agent. We are always ready to use any other that can be shown to be preferable; but as far as I have gone carbolic acid seems to possess, for the great majority of cases, the most convenient properties. It is exceedingly powerful in destroying bacteria; it is volatile, which is a great advantage for providing an antiseptic atmosphere; it is soluble in a great variety of materials—so different, for example, as water, the

fixed oils, and melted resin. This faculty of blending with materials of various natures gives carbolic acid great powers as a detergent. It penetrates through greasy matters with perfect facility. If some greasy dirt exists between the teeth of your forceps, or, say, is on the surface of the skin, or in its hair-follicles, carbolic acid will penetrate and purify this grease in a way which most salts which are soluble in water cannot do. On the Continent some of our earnest German friends, before they operate on a part, carefully cleanse the skin with ether and soap and water. That is altogether unnecessary. Carbolic acid will do all that is needful; it will clean the most dirty skin in a few minutes. It acts, indeed, upon the epidermis with an energy which, if we do not use it pure, will make our fingers suffer seriously from anæsthesia and uneasiness; for if the acid is impure it is imperfectly soluble in water, and minute particles of undiluted acid become deposited on the epidermis, and act upon it with caustic effect. But if the article is pure it makes a perfect watery solution, indicated by brilliant clearness, and if then used of not unnecessary strength—say 1 of acid to 40 of water—it produces no such inconvenience.*

But the way in which it mixes with various materials gives carbolic acid great advantages otherwise. A new principle in pharmacy seems to have been brought out by our experience with this agent. I believe it is generally assumed by pharmacists that the strength of any given agent in its action depends upon the proportions in which it is present in the vehicle or solvent—that if the agent exists in twice as great amount, it will be twice as strong in action. Now that is true in the same solvent, but it is not true with different solvents. Thus carbolic acid is soluble in 20 per cent of water. But oil can be mixed with it in any proportion. Suppose you compare a 1 in 20 solution in water with a solution of 1 part of carbolic acid in 5 of oil, you will find that the solution of 1 in 20 of water is more powerful in its immediate action than a solution of 1 in 5 parts of oil. The solution in oil contains four times as much of the agent as the watery solution, and yet is not so powerful in its action. You can bear the 1 to 5 oily solution applied to your tongue-tip or the thin skin of the lip better than you can the 1 to 20 watery solution; and the 1 to 20 solution in oil is so bland that it can be used for smearing a catheter or lithotrite without irritating the delicate mucous membrane of the urethra, to which the 1 to 20 watery solution would be intolerable. Now the reason appears to be simply that water holds the carbolic acid feebly, and so readily lets it fly off to attack anything else. Oil, on the

* I would strongly recommend the very pure form of the acid sold under the name of Absolute Phenol, at a very reasonable price, by Messrs. Bowdler and Bickerdyke, of Church, near Manchester.

other hand, holds it strongly, and therefore lets it go with difficulty. With common resin we have the opposite extreme to water. If you melt 1 part of carbolic acid with 5 parts of resin, you find that you can hardly perceive the taste of the acid at all in the mixture. I at first supposed that some sort of decomposition of the acid must have occurred, but I found that such was not the case at all, only the carbolic acid was more strongly held by resin than by oil. Now I say that is highly interesting as a new principle in pharmacy—that the energy of action of any caustic or irritating substance depends, not merely on the proportion in which it is present in its vehicle, but also in the degree of tenacity with which its particles are held by it.

But now as to the application of this principle in the employment of carbolic acid. The watery solution, besides being cleanly, is very powerful in immediate operation, but as there is not a large quantity present in the solution its action is not of long duration. Now those are just the properties we want for a detergent application. We want something that will act powerfully for the time, but transiently. Suppose a patient comes into hospital with a compound fracture of the leg—there septic mischief has already been introduced into the wound by contact of the injured tissues with objects of the external world, and what we want is to destroy the septic organisms already present in it. We wish to do this by means of some germ-poison that shall act powerfully, but that, as soon as its work is done, shall disappear, so as to leave behind as little effects of irritation as possible. For such a purpose, then, we use the 1 to 20 watery solution. Again, in our operations we must have an agent that shall produce an antiseptic atmosphere—that shall be strong enough to purify the air in which it is diffused in the form of spray—but that shall not irritate the wound to an unnecessary degree. If the solution used in the spray were like the solution in oil, it would continue to act for a long time afterwards on the tissues. But the watery solution, acting only for a short time, purifies the atmosphere without seriously irritating the tissues.

For a permanent dressing, on the other hand, we want a preparation that will not act too energetically. It must keep the septic material out of the wound; but as long as that condition is complied with, the less it acts the better, or it will irritate the skin. Further, it must not be too transient in operation, otherwise it will lose its efficacy before the dressing is changed. And so for a permanent dressing we require some vehicle which shall present it mild in its operation, but persistent. For this purpose we use resin.

In this gauze which I have here we have carbolic acid blended with resin, with a little paraffin added to prevent the mixture

from being too glutinous. Each thread of the gauze is occupied with this resinous mixture. The resin is itself insoluble in the discharges, cannot be washed out, and then again the resin holds the carbolic acid so tenaciously that the discharge may go through over and over again without washing all the acid out. So between the two we have what we require. We have the carbolic acid present in a form in which it acts sufficiently but mildly, while it cannot be dislodged by any amount of discharge that is likely to occur in twenty-four hours, and if the discharge comes to be extremely slight, as in the later stages of a wound, you may leave the dressing unchanged for a week. Beyond that I do not generally trust it; but for a week you may trust it with absolute confidence if the discharge be small.

Now as to the manner in which this gauze is applied. We must have a sufficient mass of it. I shall make up a dressing as for a case in which I expected a large amount of discharge—as in psoas abscess. We generally have about eight folds of gauze. I now make up a dressing of nine layers, which is often a convenient form, the piece employed being as long as the breadth of the tissue. This piece, about a yard square, I fold into three, and then crosswise into three again, so getting nine folds. It is essential, if you expect much discharge, that the dressing should be large—that there should be a considerable space between the wound or source of discharge and the edge of gauze dressing, otherwise you will not have material enough to ensure the absence of putrefaction. If I were to put it on as it is—gauze, and nothing more—the discharge would come directly through the porous material, and passing over and over through one limited portion, would probably, in spite of the retentive property of the resin, wash out all the carbolic acid from that part before twenty-four hours had expired; and as soon as the antiseptic had thus been removed from that portion of the gauze putrefaction would spread in. I therefore interpose under the outermost layer some impermeable tissue to prevent the discharge from traversing the gauze directly, and compel it to travel through its entire breadth. The best article for this purpose is a very thin mackintosh cloth, which is very durable, and can be used again and again for weeks together, and thus in the long run is as cheap as it is trustworthy. I take a piece of this material as large as the folded gauze, and put it under the outermost layer. It is made of pink colour, rather than white, to make it conspicuous and show which side of the dressing is to be kept outwards. If you put it outside altogether it is liable to be displaced. Then you should take care not to make any pinholes in the centre, near the wound. This may seem a small matter, but there is

room enough for I do not know how many bacteria to go abreast through a pinhole. The time is coming, or is already come, when a statement of this kind can be made without being received with a smile of incredulity, because it is now established that these bacteria are the cause of putrefaction; and if so, the laugh must be on the side of those who saw, as distinguished from those who could not see, the truth.

There is an additional antiseptic security in the fact that we fix on the dressing with bandages of the same carbolised gauze. They are also convenient in other ways. They are the cheapest form of bandage we can use, being only one-third the price of ordinary cotton bandages; they are light, and apply themselves well to the part; and the slightly glutinous character of the material in them prevents the turns from slipping on each other.

In proceeding to open a psoas abscess, we use the strongest watery solution of carbolic acid—that is to say, 1 in 20. I pour some out into a vessel, and with a piece of gauze or rag dipped in it I purify the skin and hair-follicles. I must not expect impossibilities of carbolic acid. I give it a little time to do its work. Meanwhile, any instruments I am going to use are purified by being put into a tray containing the same carbolic acid solution. Every step must be taken in such a way as really to attain your object. Do not do anything for mere form's sake. The forefinger must be purified. See that the solution goes under the nail, and under the fold of skin beside it, for you may wish to introduce the finger to explore. In psoas abscess it is a very common thing to find a large amount of pus underneath the skin coming through a small hole in the fascia, and if you were to leave matters so you would have an inadequate drainage. After making an opening in the skin, you search with the finger for the opening in the fascia; you tear it to a larger size with the finger-tip if you do not find it large enough; and, indeed, for many purposes the finger-tip is extremely valuable as an explorer. I have known a surgeon open a large psoas abscess, using the spray and gauze apparently rightly, and then, after the bandages had been applied, take off the dressing, and, remarking "I have forgotten something," plunge his finger unpurified into the wound in order to make an exploration. He had better not have used the antiseptic means at all, because he only brought them into discredit.

In treating an abscess antiseptically we do not apply the agent to the diseased tissues at all. The carious bone, like other parts affected with inflammatory disorder, is well disposed to spontaneous recovery, if only it be freed from all sources of irritation. By opening the abscess and providing free drainage, we get rid of the inflammatory disturbance previously kept up through the nervous system by the tension

of the accumulated pus, while by our antiseptic means we exclude the more serious irritating agency of putrefaction; and supposing that the bones concerned can be kept in a state of perfect rest, which, in the case of the bodies of the vertebræ, can be readily done by strictly enforcing the horizontal posture, the chronically inflamed tissues recover through the unaided efforts of nature.

Drainage is provided for by the caoutchouc tubes of Chassaignac. In a large abscess I use one as big as my little finger, and in smaller collections of matter tubes proportionately small. The tube is steeped in the carbolic acid solution for two or three minutes. Surgeons sometimes keep them for days together in the solution before using, but this is superfluous care altogether if the tubes are new, as no living organisms can exist in the substance of the caoutchouc. But we frequently use them over and over again for different cases, and under such circumstances there may be in the interior a crust of dirty septic material, and for purifying such old tubes an hour or two in the carbolic solution will be desirable. In order to prevent the tube from being pushed in too far, we have two loops of carbolised silk attached to the orifice, each of them knotted at its extremity. The orifice of the tube is placed on a level with the skin, and when the dressing is bound down it gets a purchase on these knotted pieces of silk stretched upon the skin, and so the tube cannot leave its position. If the direction of the tube has to be oblique, we cut the orifice obliquely in proportion, so as to have it perfectly level with the surface. If the tube projects, it gets bent, and fails to convey the discharge properly.

The opening in the skin is made merely sufficient for the admission of the tube. Like others, I used to be a strong advocate for a free incision, believing that if that was not done we should have sinuses formed. But that is now no longer necessary with drainage tubes and antiseptic arrangements. The tube does far better than a free incision. The latter might have its edges glued together with lymph or clots, and so be altogether inadequate, but with the tube this cannot be. And while the pus or serum is taken freely away, being prevented from putrefying, it is itself unirritating, and no longer provokes, as formerly, a copious further effusion. The smallness of the incision is, of course, in various ways a great advantage. In the case of a young lady, for instance, with an abscess in the neck, we wish to avoid making any mark. We used to make a free incision, but now a puncture, just large enough to admit a small tube, will be quite sufficient, and will leave a punctiform scar which nobody can observe.

Now I will suppose, in the case of a psoas abscess, that the

incision has been made, the purified finger has broken down any barriers interfering with the introduction of the tube, and the tube has been passed in, but all this has been done under the spray, of which I have not yet spoken in detail. Here is the apparatus. The spray consists of the 1 to 20 solution of carbolic acid mixed with the vapour of boiling water. This, as it rushes out of the small orifice in the tube, which proceeds from the boiler, sucks up the solution and carries it over the wound. The lower end of the tube which conveys the solution should have a piece of sponge in it to filter the fluid, otherwise the small orifice will soon get choked up with dust. But with this arrangement it never will get choked, although the sponge will have to be renewed occasionally. Now you must take great care that the spray is not merely steam, for a spray of steam alone would be worse than nothing at all. As you are well aware, high-pressure steam at a short distance from the point of issue is cold, and therefore has no destructive effect upon organisms such as boiling water would have, and hence it would only project successive portions of atmosphere upon the wound, with its septic particles in full activity. Therefore, it would be far better to let a wound take its chance of any organisms that might fall from the atmosphere without such a spray of steam only. Make sure, then, that you really have carbolic acid solution in the spray. This is easily ascertained by alternately compressing and relaxing the tube proceeding from the solution. Here, the tube being compressed, we have a jet of steam only, and you see its blue colour, indicating a high degree of pressure. Now, if I relax the pressure of my finger upon the tube, and allow the solution to be sucked up by the issuing steam, you see we get a much larger cloud, white in colour. It is a cloud so fine that in its further parts it is hardly more obscure or more wetting than a mist, and if there be no draught to disturb it, you may safely trust these drier and more transparent distant portions. You will then find that it does not obscure your operations, and does not moisten your shirt sleeves. It is also a respirable spray. I do not mean to say that you would select it to breathe in, but it is not intolerant to the assistants, and if you are operating on the head or neck there is no necessity to protect the patient's lungs. He may breathe it without being the worse.

The abscess being supposed to be evacuated and the drainage tube introduced, I now put on the dressing. The deepest part of the gauze must be dipped in the carbolic solution, because the dry gauze is not strong enough at the temperature of the air to destroy the septic matter that comes in contact with it. The watery solution does for this. Under ordinary circumstances we use 1 in 40, as less irritating; but in a case of psoas abscess,

such as we are supposing, I use 1 in 20, because the avoidance of putrefaction is a matter of life and death to the patient; and as the acid does not come in contact with his tissues, the only inconvenience of using 1 to 20 is to my fingers. If I dip a dirty rag in 1 to 40 or 1 to 20 solution, I know that it is at once pure itself and fit to purify other things with. This is a great demand to make on the antiseptic, but it complies with it. But if I take a bit of dry gauze that has been lying exposed to the air, or perhaps on a dusty table, and put it on the orifice of the wound, that will be just the way to make putrefaction occur. The deepest part, therefore, must be soaked in the lotion, and this is most conveniently done in the form of a separate or loose piece of gauze folded together; and where the discharge is likely to be great, as immediately after opening a large abscess, I should also moisten the middle of the principal dressing with the solution. This being done, I proceed to put it on, the middle of the dressing being opposite to the situation of the wound; and then you observe that, thanks to the mackintosh, I get the same advantage from this comparatively small number of folds of gauze as without the mackintosh I should get with a mass as thick as the distance from the wound to the edge of the dressing. Then I put on the bandage in the ordinary fashion. If the dressing is in a part where there is much movement, as the groin or the neck, I find great advantage in using an elastic bandage round the edges of the dressing, drawn tight enough to bring its elasticity fairly into play. We use safety pins for attaching the bandage to the dressing, because the gauze is of so loose a structure that an ordinary pin may travel about in it—and a patient's life may often depend upon a pin.

On removing the dressing the day after the opening of the abscess, you will find a discharge of red serum soaking the gauze, and you may have to change the dressing every day for three or four days. The discharge, however, will get steadily less; and when at the time of the usual visit you see no appearance of it at the edges of the folded gauze, you do not disturb the dressing, and the time will come when you may leave it for a week, but not beyond that time. Now, in changing the dressing you use the spray again; and here you must take care that the spray really gets under the dressing at the moment when it is raised. When you have got all your arrangements complete, do not let any one make movements in the vicinity. Let everything be perfectly tranquil, so that the spray shall not be changed by a breeze. Sometimes assistants are so impressed with the importance of their work that they are perpetually sighing, but these sighing philosophers must be requested to sigh away from the spray. When you are

removing the drainage tube to cleanse it, be especially careful that the spray is really over the wound. As it is withdrawn air of some kind must go in, and if it be not the spray it will be septic air.—*Dublin Journal of Medical Science*, August 1879, p. 97.

25.—ON THE PREVENTION OF BLOOD-POISONING IN THE PRACTICE OF SURGERY.

By WILLIAM S. SAVORY, Esq., M.B., F.R.C.S., F.R.S., Surgeon to and Lecturer on Surgery at St. Bartholomew's Hospital.

What is to be understood by the term blood-poisoning? I shall employ it in its comprehensive sense to express the sum of the effects produced by the introduction of matter charged by the action of septic poison into the blood. By septic poison I understand matter capable of producing or promoting putrefaction. This septic matter is thus, of course, separated from all inorganic and vegetable poison commonly so-called, and by its effects, at least, in the economy, from those zymotic poisons which produce such diseases as small-pox, scarlatina, measles, and the like. But I need hardly add that I include in this view not only the affection called pyæmia, septicæmia, ichor-rhæmia, and so on, but some others which perhaps would not be so universally accepted to be within the pale, as, for example, erysipelas, certain forms of erythemia, boils often, and carbuncle, and some other affections more vague and transient, which perhaps I need not now further particularise, for on this subject I have already spoken elsewhere. Of course I do not forget that concerning the pathology of erysipelas, for instance, contrary views are still entertained. For my part, I consider it to be fairly well established that erysipelas is due to some infection of the blood; that it is the result of some form of blood-poisoning, and very closely allied to the affections called septicæmia and pyæmia. Nor shall I now trouble you with any discussion of the relation of the affections called septicæmia and pyæmia to each other. But, in order to be clear, it is as well to say that I do not think there is any evidence at all sufficient to show that these affections are different in kind or in nature. So far as I can see, they differ only in degree—a degree probably determined, at least chiefly; by the intensity of the poison which operates. I can find no reason for assuming a difference in the nature of the poison itself. For the grounds of this opinion I must refer you to former papers on the subject.

The cause, then, of the blood-poisoning of which I speak is the introduction of putrid matter to the blood. I say its introduction, for if it ever originate in the blood itself, such an

origin is at present beyond research. We know nothing of it. All that we do know is that it may be introduced from without. The current belief—may I say the established fact?—is, that the agents of putrefaction exist in the form of solid organic particles, some so minute as to be beyond the range of the highest powers of the microscope, which float freely in the air, and are now commonly called germs, and which abound in decomposing organic matter of various kinds, notably—and this is of chief interest to us—in decomposing animal fluids.

Let it be observed, however, in passing, that although the germ theory in its present form is of very modern growth, the belief that the admission of air to recent wounds is in some way injurious is not a new one. It extends at least beyond the era of the introduction of subcutaneous surgery, which is founded on it.

Now, it is necessary for me, in order to avoid being misunderstood in what I have to say hereafter, to state thus much; but I do not propose to enter further into it, to disturb the vexed questions that arise therefrom. I am considering the subject now only from a clinical point of view. Therefore I turn from any farther inquiry into the more precise nature of these septic particles. In what sense, for instance, they are entitled to be considered alive need not trouble us here, or whether they are more akin to vegetable or animal life. That they are organic, and possessed at least of the fundamental attributes of living matter, seems proven by their behaviour, and by some of the tests that we can apply to them. It is enough for us that they are septic; that they can produce and promote putrefaction; and further, that fluids so changed by them may provoke those terrible effects which are only too well known as blood-poisoning. I speak thus of the fluid which poisons, saying changed by, rather than charged with, bacteria or their germs, according to evidence; but I need not now consider this question. You will remember that it was discussed in a masterly manner last year by Dr. Roberts, in his Address on Medicine. Neither have I now anything to say concerning the particles of matter at present called bacteria. Everyone knows how they are prone to abound when putrefaction and kindred changes occur. For the most part, I suppose their presence may be regarded as the signal that such changes are in progress; but the exact relation they hold to such changes is at present with many a vexed question, and it is not necessary now to disturb it.

Two conditions at least are required for the mischief of blood-poisoning; and of late, in the concentration of attention upon the other, one of these has been strangely overlooked. There must be a fluid potent for evil, and there must be a sur-

face ready to transmit it. Obviously, then, the means of preventing mischief, of conducting our patient safely through recovery from wounds and injuries, lies in fulfilling the opposite conditions: in the rigid prevention or exclusion, so far as it is practicable, of all putrefying or decomposing or foul fluid, and in keeping wounds in as healthy a state as possible, in fostering the soundest and most rapid process of repair. If either of these conditions be fulfilled, the patient may escape the mischief we dread; but undoubtedly the duty of the surgeon is, by all means within his power, to secure both. He is below the mark if he can ever be satisfied with less than this.

Furthermore, be it remembered that these two conditions, as the rule, are found in company. The fresh fluid upon the surface of a wound which is healthy and in process of repair is innocuous. No mischief of this kind lurks in fresh healthy pus; and if the fluid which bathes the surface of a wound become foul or putrescent, the character of the wound itself is apt to change, and the integrity of the surface to suffer. Therefore, a healthy wound in process of repair, and a fluid on the surface which is innocuous, as the rule, go together.

Animal fluids exposed in open vessels to the air after some time become putrid, the length of time varying with the state of the air; and so it is forthwith concluded and argued by many that if fluid upon the surface of a wound be exposed to the air it must while there become putrid too. But the fact is, that anyone who cares to witness it may see fluid at any time upon the surface of exposed wounds which is not putrid. And the explanation of this most familiar fact is simple enough: that the fluid in the vessel has been kept until it has become foul, while the fluid upon the surface of a wound in process of repair which is duly watched and properly managed is ever being renewed. The same fluid ought not to be allowed to remain long enough to undergo mischievous change. There is after all some difference between stagnant water and a running stream.

Dr. Roberts, in his memorable address last year, said: "Before we can understand the pathology of septicæmia, we must have clear ideas on the relation of septic bacteria to our bodies. We see in our laboratories that dead animal tissues, when exposed to ordinary air or ordinary water, invariably breed septic organisms; in other words, contact of septic germs with the dead tissue never fails to produce septic inoculation. But this is quite otherwise with the same tissues when alive and forming part of our bodies. You cannot successfully inoculate the healthy tissues with septic bacteria. It has been proved over and over again that these organisms, when separated from the decomposing medium in which they grow, can be injected in quantity into the blood or tissue of a healthy animal, or applied to a sore

on its skin, without producing the least effect. The healthy living tissues are an unsuitable soil for them—they cannot grow in it: or to put it in another way, ordinary septic bacteria are not parasitic on the living tissues.”

“This fact,” he continues, “is of fundamental importance in the discussion of the pathology of septicæmia. We have a familiar illustration of its truth in the now common practice of subcutaneous injection; every time you make a subcutaneous injection you inject septic germs into the tissues.”

[Mr. Savory gives a very interesting table showing the statistics of blood-poisoning after operations in St. Bartholomew's Hospital. He then says:]—

What, then, is here shown? That in 1876 the absolute number of deaths from pyæmia after operation were 2, which is at the rate of 0·49 per cent.; or, including erysipelas in the common term of blood-poisoning, they were 5—at the rate of 1·24 per cent. In 1877 the number of deaths from pyæmia were 4—at the rate of 0·95 per cent.; or, including erysipelas, they were 6—at the rate of 1·43 per cent. In 1878 the deaths from pyæmia were 4—at the rate of 0·96 per cent.; with erysipelas, 7—at the rate of 1·68 per cent. Once more: During the three years there was a total of 18 deaths from blood-poisoning after 1235 operations, and this is at the rate of 1·44 per cent.

Now, so long as any instances whatever of this mischief occur, we shall regard our work in this respect as defective, and strive to do it better. I, at least, am ready to change my present plan of management when there is evidence of this character that better results can be accomplished by any other means.

Yes, I think it cannot be doubted that the occurrence of blood-poisoning during the progress of wounds and recovery after operations has been of late very far less common than formerly, and is, I venture to believe, daily becoming yet more rare. It is far less frequent now than it was, I will not only say many years ago, when we cannot estimate its frequency from lack of any adequate record—because, indeed, then its actual occurrence often escaped even passing notice—but it is now far less frequent than it was only a few years back, within even the memory of younger surgeons. But congratulatory, I had almost said triumphant, as are the statistics of the present day, I must own that it is necessary to regard these with a jealous eye, and in many instances at least not to accept them without reserve. For this chief reason. Since the prevention of blood-poisoning after wounds and operations has become the great theme of surgery, surgeons almost everywhere have been naturally and very laudably anxious to gain credit for the best results. A keen, active, and noble rivalry has sprung up

between surgeons and between institutions. A splendid struggle now goes on throughout England, Europe, and the civilised world to reduce the mortality from this cause after operations to the lowest possible terms. Beyond all doubt, the gain to human life and health from this has already been immense—who shall say how great, or how much greater still it will be? But then, you know, in such a case it is almost inevitable but that the judgment will be swayed by the desires; that in one's anxiety to escape what happily has become almost the disgrace of the occurrence of blood-poisoning in any case under one's care, the signs of it are rather open to be misinterpreted, or explained away, or called by some other name. The mischief that arises and perhaps kills is set down too readily to some other cause—to any cause rather than this. In short, it is met as a disreputable acquaintance and not recognised. But, gentlemen, even this is by no means without excuse, if only such sophistry is the outcome of enthusiasm and does not willfully pass beyond the limits of honesty. It leads nevertheless to error, and of course is to be avoided if possible. For my part, at least, then, I do not accept statistics here (or, for that matter, statistics of any kind) without reserve. Statistics on this head, to be fairly trustworthy, should state not merely the fact of death, but in each fatal case after operation or injury, when not attributed to blood-poisoning, a plain statement of what is actually disclosed by an examination after death should be given, and this would be yet more valuable if such a record were made by some independent observer. Or, failing this, I would rather take as a standard the total number of deaths which occur after injury or operation, to whatever cause or causes they may be attributed; for, excepting distinctions which can be safely drawn, such as immediate death, or death supervening within an hour or so, and obviously from direct exhaustion or death from hemorrhage, and excluding the results of certain operations—as tracheotomy and trephining and those for hernia—in which death is often due rather to the disease or injury for which the operation is performed than to the operation itself, I should venture to say the difference is very small indeed between death after operation from any assigned cause whatever, and death from poisoning of the blood.

And now, furthermore, by what particular means have these results been achieved? Hitherto, I say advisedly, the best results by the simplest means.

Forgive me if I refer again to the records of my own hospital—St. Bartholomew's—for evidence of this responsible statement. I claim from our own statistics this—and if I am in error you will reject the claim—that the result to which they testify has

not been, on the whole, surpassed. I believe they may challenge comparison with those from any other trustworthy source.

Now, considering all the various details in the treatment of wounds and the management of cases after operation, the practice of no two surgeons of the staff is precisely alike. Each one of the surgeons perhaps carries out certain details in some way different from the others, but on the chief objects in view we are undoubtedly in accord. We aim at the most scrupulous cleanliness, in the most comprehensive sense of the term. We strive to secure this by all possible means. We watch very carefully the actual state of wounds, and we use very freely antiseptics of various kinds. And with cleanliness we attach for the most part the highest importance to rest. We are careful to disturb wounds during the process of repair as little as possible. Cleanliness in its surgical sense and rest in its physiological sense may be said to be the leading aims. But we are by no means satisfied with directing attention only to wounds. We are most jealous of the state of the atmosphere of our wards. We keep the air as pure as possible. We have no particular or partial system of ventilation. Ventilation is effected only by open windows and large chimneys. But we are proud of the habitual state of our wards, even when most closed, as during the night-time. We should not for an hour tolerate any unpleasant or suspicious odour. We are very careful to avoid, as far as practicable, any tendency to overcrowding of wounds in the same ward, and each patient has from 1100 to 1400 feet of space. Lastly, we attach the highest importance to the state of health and condition of our patient before operation, and never, when we have choice and opportunity, inflict an injury without previous inquiry in this direction very fully carried out. Well, what of all this? will perhaps be said. Of course, all this is done everywhere. So much the better. The results, you see, are not due to anything beyond the observance of the recognised principles of surgery. I have said our individual practice differs in detail; but my conviction is, that our results are due to the strict observance of sound principles of surgery. For the rest, I shall take the liberty of trying your patience for a few moments by describing somewhat more in detail the practice I follow, because I believe that results equal to the best hitherto obtained may be thus reached.

Well, then, taking a case—say, of amputation through the thigh, or of excision of the breast—I should treat the wounds in the way following. Having carefully arrested all hemorrhage, using most probably the carbolised catgut ligature, and having gently removed any particles of blood-clot that may have lodged on the surface, employing only clean water or

sponges just rinsed out of it, I should, without any further interference with the surface of the wound, bring the edges together, adapting these as nicely as possible with silver-wire sutures. I should not in this way attempt to close the wound completely, but I should leave spaces between the sutures, perhaps from one to two inches long. Then, over the course of the wound and for some distance on either side of it, I should place a layer of folded lint which had been previously well soaked in olive or almond oil containing one part in about fifty of carbolic acid. Over this again I should place two or more layers of dry lint, either with or without cotton-wool; so arranging this as by gentle and equable pressure, to secure, without any violence, as far as practicable, the accurate adaptation of the surfaces of the wound throughout, avoiding thus any considerable cavity in the interior. I should secure all this by strapping or bandage, or both, so adjusting these that they may be hereafter removed with the least disturbance. I should place the patient and the wound in the most comfortable position possible, having especial care to the fact that fluids as they form may flow outwards. Thus for instance, after excision of the breast, I have, for some time past, placed the patient, not on her back, but on the opposite side, so as to make the inner angle of the wound the most dependent part of it. I am convinced that this assists greatly in promoting speedy and satisfactory repair. It is much better for fluid to escape at the inner than at the outer angle, and this more especially when the axilla has been disturbed by the removal of glands. The cellular tissue is so very much less abundant and less loose towards the sternum, that the chance of infiltration of the fluids is very much less. As a rule, perhaps, I do not disturb this arrangement for forty-eight hours, although very often I change the dressing and inspect the wound after twenty-four. I am guided in this matter of time chiefly by the state of the patient; whether spare or full-bodied; her sense of local and general comfort, freedom from or complaint of pain; and the season or temperature. But whenever I am in any doubt, I change the dressings. These, then, are removed with the utmost gentleness, and the state of the wound carefully inspected. Especially is attention directed to whether there is any tendency to the lodgment of fluid; whether that which forms can escape freely; whether there is much tension of the edges. I am bold enough to think that any surgeon who understands his business can tell, without any painful handling, whether the surfaces of the wound are fairly in contact, or whether there is any tendency to the accumulation of fluid separating them. But if any doubt arise on this important point, a perfectly clean probe or director lightly applied to some portion of the wound will solve it, and secure

ample vent; if at all necessary, I should not hesitate to remove one or two or more sutures. If the wound presented no other evidence than that of satisfactory repair, I should dress it as before, and proceed in this fashion, dressing and examining it daily or less frequently, according to circumstances. But if at the first dressing, or whenever afterwards the discharge became at all profuse, or the surfaces did not remain in contact, or there were much tension or a blush at the edges, I should forthwith substitute a bread-and-water poultice for the previous dressing, and probably continue to apply this until at least all the deeper portion of the wound had closed. When I dressed the wound, I should wash it probably from the first with tepid water, perhaps containing some permanganate of potash in the form of Condy's fluid or other potent antiseptic of the least irritating kind. I should accomplish this washing out, if I thought fit, of portions or even the whole of the interior by the use of a syringe, avoiding contact of sponges or other substances with the wound. I aim here at the utmost possible cleanliness, having at the same time due regard to the avoidance of any unnecessary disturbance, that the process of repair be not interrupted. And withal I endeavour, by means I need not indicate, to secure for my patient the most complete rest and the purest air.

With regard to the substitution of a poultice or water-dressing, or some other form of application for the simple dressing used in the first instance, I think one can tell for the most part beforehand if they are likely to be required at all, or early in the management of the wound. In young persons in tolerably good health and spare, most wounds, when not worried, heal very directly. It is in those advanced in life, with flabby textures and much loose fat among them, that wounds give most trouble. These pour out fluid freely and are prone to flag, and, while the process of repair is delayed, may fall into mischief in various ways.

Now, I am sure you will allow that this mode of treating wounds in general which I have thus slightly sketched is characterised by its simplicity and the entire absence of all novelty; my purpose being to interfere in the least degree possible with the work of nature. Some years ago it would not be worth recording; and now perhaps it will excite surprise to find that any plan of treatment with so little in it is still adopted. Allow me yet to trouble you with a few comments.

In the first place, at the time of the operation, or immediately afterwards, you see, nothing is applied to the wound but water as a rule; no antiseptic of any sort, provided the surface of the wound is healthy. Because I believe that such healthy natural surfaces are in the state best adapted to satisfactory repair; and that, as a rule, in proportion as they are changed by the appli-

cation of foreign agents, so are the changes which initiate repair hampered or arrested. I would ask, if it be wise thus to wash the surfaces of recent wounds with antiseptics, why not treat all wounds so—as for example, in the operation for hare-lip? Antiseptics, at this time especially, are irritants in some degree; and unless they are very strong, and therefore very irritating, their power to arrest mischief is lost before the period for that mischief has arrived. Therefore it seems to me, for the employment of antiseptics from the first to become reasonable, the wound throughout its progress must be kept thoroughly under their influence.

Now I must pass on to speak of that particular plan of practice which aims at unconditional security—the plan the purpose of which is to exclude all risk of blood-infection by the rigid exclusion of living germs; notably of that particular method which has been introduced by Lister, and at present known everywhere as Lister's method. Now, the relative value of this method of treating wounds may be tried—should be tried, I think—first by the facts which have been ascertained in regard to it; and, secondly, by the arguments which can be advanced for or against it.

First, then, with regard to actual facts. Are there any trustworthy statistics to show that hitherto the results obtained by Lister's plan are better than the best results obtained by any other method? I think most will admit that this is a question of considerable weight. Well, I take, for instance, our hospital statistics, to which I have already referred, on the one hand, and I seek in vain for any parallel results on the other; and, while this is so, I shall consider that I am justified in the conviction that hitherto the best results have been achieved by the simpler method. And I must add that one seeks in vain for statistics of any kind from sources from which I submit we are entitled to expect them. Why are such statistics withheld? Are they not worth the trouble of collection? And, if they have been collected, why are they not published? But let this pass. I say I know of no results from Lister's method like those which I have given.

The adoption of Lister's plan has effected a vast improvement in the death-rate of a particular institution; the results obtained by it are still far below those which have been obtained by other methods. The contrast between the results of Lister's plan and the records of what I would fain hope may now be called former days is most marked where the previous mortality was highest; and it is easy to understand why the most enthusiastic reports in its favour come from those places where the sanitary conditions are worst. Beyond all question, I should say, in too many instances, it has proved far better than that which it has replaced; but to conclude from this that it is

better, or in its results equal to every other plan at the present time adopted, is to set the simplest rules of logic at defiance, to foster error and confusion. No; Lister's plan must be tested (it would be an insult to its author to propose less), not by contrast of former with present statistics, but by comparison of it with the best results which now are obtained otherwise.

I cannot admit the claims of Lister's method; because, although undoubtedly very good results are to be obtained by this practice—better ones, no doubt, than most of those which were reached in former years, or are still in many places—yet that it has not shown results superior or equal to those which have been otherwise achieved; that it has, moreover, grave drawbacks from which simpler plans are free; that if it fail, it is worse than useless by increasing the risk; and, therefore, that it has not established any title to supersede all other methods in the practice of surgery.

The principle of Lister's practice is an easily intelligible, and therefore very attractive, one to the public—I mean the more educated portion of it, even to men of scientific attainments, who have little or no knowledge of clinical surgery; for the one fact can be seen so plainly, while all other questions which are forced upon the attention of the surgeon are shut out from them.

But the principle on which it rests is a sound one; the logical outcome of established facts. Granted most freely and fully so far as in this direction it goes. But is every other plan of treatment without principle and opposed by logic? Let me recall your attention to some words which appear to me to be about the wisest which have been spoken on the subject. Last year, in his address, Dr. Roberts said: "We should probably differ less about the antiseptic treatment if we took a broader view of its principles. We are apt to confound the principle of the treatment with Lister's method of carrying it out. The essence of the principle, it appears to me, is not exactly to protect the wound from the septic organisms, but to *defend the patient against the septic poison*. Defined in this way, I believe that every successful method of treating wounds will be found to conform to the antiseptic principle. Take, for example, the other method of treating wounds which is sometimes compared in its results with Lister's method. What is this treatment but another way (only less ideally perfect than Lister's) of defending the patient against the septic poison? Because, if the surgeon succeed in providing such free exit for the discharges that there is no lodgment of them in the wound, either they pass out of it before there is time for the production of the septic poison, or, if any be produced, it escapes so quickly that there is not enough absorbed to provoke an appreciable toxic effect."—*Medical Times and Gazette*, August 16, 1879, p. 174.

26.—ON PAIN AS A SYMPTOM OF FRACTURE.

By JOHN DUNCAN, Esq., M.A., F.R.S.E., Surgeon to the Royal Infirmary, Edinburgh.

My object is to give precision to our estimate of pain as a symptom of fracture. Its value has, I believe, been generally under estimated, and it is certainly desirable that for this as for all the signs the value should be carefully determined.

We shall find that pain may be useful to us in two ways. It may clear up cases that otherwise would be exceedingly difficult, and in easy cases may save the patient farther suffering in the process of investigation.

Pain may present itself in connexion with fracture under three varieties: 1, as such, pure and simple; 2, as pain on motion; 3, as pain on pressure, or tenderness.

These three varieties are usually concomitant at the site of fracture. But they are so also in many other injuries, and as positive evidence this concomitance is not of much significance. It may happen, however, that its absence is worthy of note. Thus, there can be no fracture if pain be not felt on moving the part, even though the other forms of suffering be very prominent after an injury. Or, again, with some fractures, although it is more common with dislocation, pain is referred entirely to a distant part of the limb. In these the absence of pain on pressure and motion proves that the injury, whatever it may be, does not lie at the spot to which sensation is referred. It is not unfrequent to have a dislocation of the humerus supposed to be a sprain of the wrist, and I have seen a fracture of the neck of the humerus missed from the same cause. This error could not have arisen if the point I have referred to had received due attention.

But according to the circumstances under which it occurs, its position, severity, and combinations, each variety of pain is capable of affording valuable information. Let us take them in their order.

Pain alone, and simply as such, is certainly not often of much significance, even though we leave out of account the possible difficulties arising from referred sensation. In the case of direct violence, pain at the injured spot indicates merely the locality of the injury, and the same may usually be affirmed of injuries produced by muscular action. In the one, bruising of the soft parts; in the other, rupture of muscular or fibrous textures is not thus to be distinguished from fracture.

In examples of injury from indirect force, the reliance to be placed on this symptom varies with the part injured. In the neighbourhood of a joint it may mean strain or dislocation, but the more nearly its site approaches to the line of weakness in

the shaft of a long bone, the more probable is it that pain is produced by fracture.

It never, however, becomes pathognomonic. Other symptoms are required for confirmation, are generally more reliable, and sometimes are as easy to ascertain.

The pain produced by motion may be of great use. It is logically evident that if we can impress motion on an injured but healthy bone by means of force applied at a distance from the part hurt, and without in any way disturbing its surroundings, the pain so elicited must mean fracture and nothing else. Practically this is not always possible. Force may be applied to the injured bone either as a cross strain or by pressing the ends towards each other. By the former method tension is also most probably brought to bear on the surrounding soft parts. By the latter the result may be vitiated by the neighbourhood of a joint, and in the shaft of a bone is usually rendered of little value by the fact that other symptoms are more distinct and as easily ascertained.

Nevertheless, there are circumstances in which the presence of pain on motion becomes important. Let me mention one or two.

It is not, perhaps, a matter of much moment whether a blow upon the chest have led to fracture of the ribs or not, and inasmuch as the treatment is similar for severe bruise and for fracture, surgeons are often content to leave the question undecided. An accurate diagnosis, however, may in medico-legal cases be important, and is always satisfactory both to surgeon and patient, if it can be made without injury to the latter. I think it possible to do so by means of the pain elicited by motion, when it would be unjustifiable or difficult to bring out such symptoms as crepitus or increased mobility. Having ascertained that the pain on pressure of the injured part is situated over one or more ribs (if it be not so, there can be no fracture), the surgeon should trace these ribs to a distance of some inches. When at this distance from the bruising, firm pressure is made, severe pain will be developed at the fracture, if there be fracture, little or none if there be not. It is only necessary to be accurate as to the bone pressed upon.

Again, fracture of the neck of the femur often gives rise to difficulty in diagnosis. The depth and other conditions of the fracture make it sometimes far from easy to ascertain the crepitus and abnormal mobility, and we therefore rely largely on the deformity, as ascertained by careful measurements and comparisons. I believe that the most experienced men are occasionally deceived. There exists a well-known class of cases, in which, although the absence of eversion and shortening has been at once accurately determined, it is discovered (perhaps

after the patient has been moving about with difficulty for months) that there is shortening often to a considerable extent. The explanation commonly given is that chronic interstitial absorption of the neck of the bone has taken place. I do not think that this has been well made out. I would not deny the existence of this interstitial absorption, but it is not easy to understand what physical injury other than fracture would be sufficient to set it up. I believe that a fracture, in which the surfaces are crushed into one another, is more frequent here than is generally supposed, and that in some a considerable amount of absorptive action ensues. But I know also that shortening sometimes comes otherwise than by absorption. I have on several occasions met with cases, of which the following is a type:—An elderly man was admitted to the Infirmary last November, having been knocked down by a horse at Hallow Fair. He had a fracture of the lower end of the right radius, and complained of severe pain in the left hip. There was no shortening, no eversion, no crepitus, but, from the character of the pain, I had a strong suspicion of fracture of the neck of the femur, ordered him to be kept in bed, and had his leg measured once a week. The third measurement showed a shortening of one inch, which had, therefore, occurred between a fortnight and three weeks after the accident. I have notes of two cases in private practice in which shortening similarly took place in the third and in the fourth week respectively. The only explanation plainly is, that there was fracture which had yielded to muscular action when softening of the impacted parts had sufficiently advanced. Now, the true way to save the surgeon's credit is to make a correct diagnosis at the first—not to hedge by stating that there is only a bruise, but that such bruises are prone to produce shortening. This diagnosis may be made in two ways. One method consists in giving chloroform, and so moving the leg as to elicit crepitus, when you will also probably find that you have succeeded in producing shortening and eversion. The other, and, as I think, the better mode is, having weighed well all the circumstances, to confirm the diagnosis by the character of the pain produced by movement. I have not yet satisfied my mind by a sufficient number of cases, but I am nearly certain that, if the pain caused by inversion be considerable, a fracture has to be dealt with. Pain on eversion and flexion are also greater with fracture than with bruise, but the distinction does not appear to be so marked. No doubt, in a case of chronic arthritis a bruise cannot thus always be distinguished, but the history and concomitants must then be called to our aid.

Many other fractures have special characteristics in reference to the motions which produce pain. I have not worked out the details, because they appear not of such practical importance as to require individual mention.

As a symptom of fracture, then, pain on motion is generally of value only in so far as it confirms others. Sometimes it rises to a more prominent position. Often its absence gives assurance of the absence of fracture, for if it be possible to impress motion on a fractured bone it is certain that pain will be produced.

Tenderness or pain on pressure over the injured part is invariably present in all cases of fracture. It may be definitely affirmed that this tenderness is experienced round the whole circumference of the bone in the line of fracture. The question is, How can this law be practically applied? Its usefulness varies according as the fracture has been produced by direct or indirect violence, and I think it will be found that as a negative sign it is decisive, if not always clinically valuable, and that as a positive indication of fracture it is also of great importance.

In direct fracture from the depth and situation of the bone, it may not always be possible to apply the test with precision, and even when the bone is partly superficial, that is precisely the portion of its contour most likely to be covered by tissues which the blow has also bruised. But it may be that the whole bone can be felt, or that the opposite side from that struck is in a position which can be reached. If it be so, the presence of pain on pressure indicates fracture, and its absence proves that there is none.

But in fractures produced otherwise than by direct violence, pain on pressure attains its highest value. The same circumstances as in direct fracture, no doubt, impede more or less the precision with which the fact can be ascertained. The bone may be so deep or so situated that it cannot be directly pressed upon in the whole or some part of its circumference, and injury of the soft parts in the neighbourhood may under certain circumstances be produced by indirect violence. But these impediments are not so serious as in the former case.

The first impediment does not interfere with the negative value of the sign in injury from indirect any more than from direct violence. If any part of the contour of a bone be subjected to pressure without pain it is certainly not broken on that particular level. And this is of practical value especially in the vicinity of some of the joints. The elbow, for example, has been violently wrenched. By putting the arm in the easiest position, and then running the finger along the exposed edge of the ulna, pressing together the condyles of the humerus and their ridges, and feeling the head of the radius, you may distinctly determine that there is no fracture without further examination. And, on the other hand, the depth or situation of the bone does not necessarily impede the discovery of fracture by this sign. It is not essential that the whole circumference of the bone should be exposed to pressure. A very

small portion will suffice, if it be immediately under the finger, if the painful spot be in the usual line of fracture, and if the cause of the injury be undoubtedly indirect. Take a common example of fracture from indirect violence, Pott's fracture of the fibula. Even though there be no displacement, the diagnosis of fracture may be made with certainty, if there be pain on pressure above the external malleolus, and especially if this be accompanied by tenderness over the internal lateral ligament of the ankle-joint. It is only, therefore, when the bone is altogether so situated that the pressure applied loses in precision that this circumstance interferes with the usefulness of the symptom.

The other impediment, that, namely, of injury to the soft parts from indirect violence, is not, in most cases, one of much moment. In the neighbourhood of joints it must be borne in mind. In the shoulder-joint there are fibrous expansions of the tendons, extensions of the joints, capsule, and bursæ, over the bone, which are prone to be injured and become sore to the touch. There is also naturally a tender spot over the anterior tuberosity, which becomes much more painful in disease or after injury. In this region, therefore, no reliance, or very little, can be placed upon this sign. In the knee, also, the curiously tender spot over the lower part of the inner condyle should not be allowed to lead to confusion.

But with a few well-defined and easily-ascertained exceptions of this sort, the proposition may be maintained, that if the violence be indirect, pain on pressure over a bone indicates fracture, its absence negatives the possibility of the bone being broken. That does not mean, of course, that this sign is always to be placed in the front rank. What I have called the perspective of symptoms must be maintained, and it varies for each case. Sometimes, as in fractures of the shaft of the humerus or femur, mobility stands first, crepitus second, and tenderness only third. In other cases deformity is of greater importance. But not rarely pain may be really the leading and even a pathognomonic symptom.

Let us consider for a moment the diagnosis of the two most common fractures of the body, that of the lower end of the radius and of the middle of the clavicle. They are both due invariably or almost invariably to indirect violence. If a person fall upon the palm of the hand he may sustain one of three injuries from the bending backwards,—a dislocation of the wrist, a sprain of the joint, and a fracture of the lower end of the radius. The first is so very rare as hardly to be worthy of discussion in the present connexion. I have only once seen it; and then, as I was manipulating to define the relations of the styloid processes, the joint slipped into position. I cannot say,

therefore, from experience, what the exact situation of tenderness is. Doubtless it is over the joint itself; but doubtless, also, the diagnosis depends on the deformity.

As to the means, however, of making a comparative diagnosis between the other two, I am absolutely certain. The characteristic deformity of Colles' fracture is marked in at least nine cases of ten, and there is required for diagnosis nothing more than careful inspection. But in the tenth case the deformity is so slight as not to be easily made out, or is masked by the general swelling. It is in the distinction of these more obscure examples from sprain that tenderness plays its rôle. If there be a fracture, the pain on pressure is more severe above the level of the styloid process of the radius, both before and behind; if there be a sprain, the pain is in the region of the joint. It is impossible to have anything more definite and precise.

In fracture of the clavicle, pain on pressure is also pathognomonic. You have only to run your finger along the line of the bone. No doubt, in all cases for purposes of treatment you ascertain whether deformity is present. But that is not necessary for diagnosis, and its absence is not a source of embarrassment. Tenderness here is the important sign; swelling and deformity come next, and no other sign—neither mobility nor crepitus—need or ought to be sought for.

I have not attempted to deal with my subject in an exhaustive manner by going over all the possible fractures in the body. I have endeavoured to show that in each case a special order of importance ought to be given to the various signs, and that in not a few, pain in some of its aspects is of great importance sometimes as a pathognomonic, sometimes as an essential, and sometimes as a confirmatory symptom.—*Edinburgh Med. Journal*, May 1879, p. 966.

27.—CASE OF COMPLETE DISLOCATION OF THE HEAD OF THE RADIUS FORWARDS.

By Dr. J. C. OGILVIE WILL, Surgeon to, and Lecturer on Clinical Surgery at, the Aberdeen Royal Infirmary.

[The following case is interesting on account of the successful reduction of the dislocation so long as twenty-four days after its occurrence. "It is brought forward as an example of a comparatively rare injury, and of a positively excellent result obtained under peculiarly unfavourable circumstances."]

A. B., aged five, when running at school, fell over a form on March 22nd, 1878. On her return home she complained of pain in her left elbow, but no attention was paid to it until the following Sunday, when, on her arm being suddenly seized by

one of her parents, she cried loudly, and seemed to be much hurt. No advice was sought until the 29th of March, when my friend Dr. James Brander was called in. He found great inflammatory swelling of the left elbow-joint, with some œdema of the hand and forearm. The inflammatory symptoms were so marked that, although he felt satisfied, from the very evident malposition of the limb, that the patient was the subject of severe injury of the joint, he did not feel justified in manipulating the parts so as to ascertain the exact nature of the case. Soothing applications were prescribed, and strict rest enjoined. The swelling of the tissues in the neighbourhood of the joint persisted, the skin became brawny-red, and the œdema of the hand remained undiminished. On April 8th he kindly requested me to visit the child in consultation with him, when the presence of the luxation was ascertained; but, on account of the inflamed state of the parts, it was not considered prudent to attempt reduction at the time, and we agreed to delay operations for a few days. On April 17th, inflammation having greatly subsided, I again visited the child, and, after chloroform had been administered, the following appearances were elicited:—Forearm semi-pronated, midway between complete extension and semi-flexion, with well-marked inclination outwards; external border of forearm slightly shortened; an unnatural vacuity behind and below the external condyle in the situation normally occupied by the head of the radius; an alteration in the direction of the long axis of the radius, which, when followed with the fingers, led to a point in front of the external condyle, where the head of the bone could be distinctly felt. On attempting to flex the forearm it was found impossible to bend it to a greater degree than a right angle, a feeling of locking being very manifest. Complete extension was readily achieved, and there was an abnormal degree of lateral motion.

Reduction was effected with some difficulty by extension and counter-extension, and thumb-pressure applied to the head of the radius. A rectangular splint was applied. Ten days afterwards Dr. Brander commenced passive motion of the joint; and on May 17th, when I next saw the patient, recovery was complete, the motions of the joint being in every way perfect.

Remarks.—Considerable diversity of opinion seems to exist regarding the commonness or rarity of dislocation of the radius forwards, Hutchinson stating that it is not uncommon, and Erichsen that it is the most usual of the three dislocations of the radius alone; while Boyer wrote: “Mais on ne connaît pas d’observation bien authentique de la luxation de l’extrémité supérieure de cet os en devant.... Nous doutons que cette luxation puisse avoir lieu sans une complication de fracture.... On ne peut donc, dans l’état présent de nos connaissances,

admettre une luxation de l'extrémité supérieure du radius en devant." Boyer's statement that this dislocation cannot occur without fracture has been completely refuted, but the paucity of cases recorded by writers of surgical works would lead to the belief that it is rare. Sir Astley Cooper only met with it six times. Malgaigne met with three uncomplicated cases, Hamilton saw it nine times. Chelius, Bransby Cooper, and Pirrie have each placed two cases upon record. Many other isolated examples have been recorded, but I only cite the names of a few surgeons who have had great experience in this department of surgery as indicative of the comparative rarity of the lesion under notice. Apart from its frequency or infrequency the case possesses features of interest. The symptoms presented by it were those generally described, but the only one to which I would direct attention is the oblique inclination of the forearm outwards, which, as I have already stated, was well marked, and which was certainly most striking. To Malgaigne must be ascribed the credit of first describing it, and he speaks of it as "*un phénomène essentiel*," notwithstanding which, with the exception of Hamilton, Lane, and Nélaton, (who gives "*l'inclinaison de l'avant-bras en dehors*" as one of the diagnostic signs between complete and incomplete dislocation forwards, it seems to have escaped the observation of other writers on the subject), some of whom give figures representing the appearance accurately enough, and who, while detailing all the other symptoms at length, yet omit to even notice the one to which Malgaigne has so forcibly alluded. In the case now under notice it was the first symptom observed, and one of the most telling of any of the signs elicited; it therefore seems to be well worthy of note, and deserving of more attention than it has yet received.

The next point of interest is the reduction of the luxation at so distant a period from its occurrence as twenty-four days. The difficulty of reducing even a recent dislocation of the kind is universally acknowledged, and has been well put by Nélaton in the following sentence, taken from his work already mentioned:—"Pour la luxation complète, la difficulté de la réduction, déjà signalée par Hippocrate, semble prouvée par la proportion des pièces pathologiques recueillies, comparée aux faits observés sur le vivant." Sir Astley Cooper failed in two cases, and Hamilton in one, on the seventh day, and in Malgaigne's collection of twenty-five cases, excluding six which had been unrecognised, manipulation failed in eleven, only eight of the entire number ever being reduced. The only case with which I am acquainted where reduction was successfully accomplished at a late period was one under the care of Gosset, who succeeded in effecting replacement three weeks after injury.

Hutchinson reduced one at a still later date, but relaxation followed. The case now recorded is, so far as I have been able to ascertain, the only one on record where manipulation was followed by permanent reposition of the bone at so distant a period as twenty-four days.

The last point worthy of mention is the after-treatment of the case, which, so far as the time of commencing passive motion of the joint is concerned, differed from that generally inculcated. The danger of relaxation taking place has been so universally admitted that it has been laid down as a rule that the injured joint should be kept perfectly immobile for a period of from four to five weeks (Chelius, &c.) In this case, however, I ventured to take a different course, for I feared that if this injunction were obeyed we would never be able to overcome the resulting stiffness of the articulation, and that immobility would be permanent, for the child was timid in the extreme, and not one likely to make use of, or to allow that interference with, her arm which would have been requisite for the restoration of the functions of the injured member. On this account I suggested to her medical attendant the advisability of practising passive movement at the end of ten days, first once a day, and then oftener, the splint being replaced immediately afterwards. Dr. Brander, agreeing with this view, and finding that all inflammatory symptoms had disappeared, and that there was no tendency to relaxation, carried out the treatment so successfully that when I next visited the patient, four weeks after the dislocation was reduced, I found that she had regained the full use of the joint, and that she could move it as easily and perfectly as she did its fellow—a result which far exceeded my fondest anticipations, and which certainly justified the departure from the hard and fast line drawn by surgical authorities when dealing with the after-treatment of dislocation of the head of the radius forwards.—*Lancet*, June 7, 1879, p. 800.

28.—ON THE VALUE OF OPERATIVE INTERFERENCE IN THE TREATMENT OF INFLAMMATION OF BONE.

By THOMAS BRYANT, Esq., F.R.C.S., Surgeon to Guy's Hospital.

DRILLING AND TREPHINING BONE THE SUBJECT OF CHRONIC INFLAMMATION.—The operation of dividing longitudinally by a saw or of trephining bone which is the subject of chronic inflammation, has been practised at intervals by surgeons for many years, although it has hitherto been confined to three classes of cases, viz :

1st. To those in which relief has not been obtained by other treatment persistently applied.

2nd. To those in which from the persistency or severity of the pain surgical interference is demanded, and—

3rd. To others in which an abscess in the bone was suspected, but chronic inflammation was alone found.

And yet if the principle on which the practice is based be right, the operation should have a wider range; for granting that the division or trephining of a bone chronically inflamed *relieves tension*, gives exit to inflammatory fluids, lessens pain, and cures disease in such obstinate cases as have resisted all other modes of treatment, it is not unfair to infer, if the same, or an allied practice were applied at an earlier period of the inflammatory affection, that equally good results would be obtained; and that while in some cases the inflammation would be checked by the operation, in many an early and complete recovery might be anticipated.

The following cases are now recorded with the view of sustaining this opinion, and I believe them to be sufficient not only to justify the practice which has been inculcated, but to indicate that it is the right one to adopt in all cases of osteitis which do not readily yield to milder treatment.

Case 2.—Expansion of the Head of the Tibia from Osteitis; drilling of the bone; recovery.—(Reported by Mr. BROWN.)—George S—, æt. 30, was admitted into Guy's Hospital on April 5th, 1871, under Mr. Bryant's care. He was a railway porter, and six months before admission struck the outer side of the head of the left tibia. Great swelling appeared at once, but with fomentations this gradually subsided, and he returned to his work. Six weeks after the accident, however, pain re-appeared in the part, and this, in spite of treatment, gradually became more severe.

On admission.—The head of the tibia is clearly much enlarged, although the skin moves freely over it; firm pressure upon the bone excites pain, and the tissues over the diseased bone feel hotter than those elsewhere. The pain, which is of a dull aching kind, is far worse at night. The knee-joint is sound. There is no history of syphilis.

Mr. Bryant said that the symptoms pointed to osteitis of the head of the tibia, which if not arrested would probably extend to the knee-joint. He believed the pain to be due to tension of the bone from effusion, and he gave it as his opinion that if the tension could be removed the pain and possibly the disease would disappear.

On April 11th the man was anæsthetised, and an incision was made over the bone through the periosteum, which was much thickened, and then the bone was punctured in two places with a drill. After the operation everything went on well, and all pain ceased. The bone gradually became smaller. The wound

healed kindly, and when the man left the hospital, six weeks later, the wound had nearly completely healed. The bone had not, however, resumed its natural dimensions, though it was not the seat of any pain.

Some months later the man was quite well.

Case 3.—Expansion of the Great Trochanter of the Femur from Ostitis and Diseased Hip; trephining and drilling bone; recovery with a stiff joint.—(Reported by Mr. C. K. SHAW.)—Walter S—, a florist, æt. 17, was admitted into Job Ward on June 23rd, 1875. He was a delicate looking boy, but had always enjoyed good health. Twelve months before admission he felt what he called growing pains in his left leg, and three months later he suffered from rheumatism in the same leg and slightly in his back, but he soon got better and returned to his work.

About seven weeks before admission his hip began to pain him when he walked, but it was easy when he rested. There was no history of any fall or blow. He came to Guy's Hospital as an out-patient under Mr. Lucas. When first seen there was a slight swelling on the inner side of his thigh which gradually increased. It had appeared about three weeks after he first complained of pain. He got worse, and at last was unable to stand without help.

When admitted his leg was in a natural position, but he could only move it slightly, and it pained him very much to do so. The upper part of the thigh was enlarged, as if from expansion of the great trochanter. The skin over the bone was healthy, but the subcutaneous tissues were much thickened. Pain was caused by any movement of the limb, and was increased by pressing upon the trochanter. The leg was slightly adducted. The thigh measured in circumference one and a half inches more than its fellow.

Mr. Bryant regarded the case as one of ostitis and periostitis of the great trochanter and of much of the shaft of the thigh-bone, the hip-joint having become affected from the extension of the disease.

Mr. Bryant's double interrupted splint was ordered to be applied, in order to keep the joint quiet and prevent any further adduction of the limb. Iron and cod-liver oil were given.

By this treatment the man improved, but the enlargement of the bone continued. The pain was also unchanged. It was consequently determined to cut down upon and trephine the bone, with a view of checking the inflammatory action in it, and thus of saving the joint.

August 20th. The patient was placed under the influence of an anæsthetic, and an incision was made, about three inches long, over the great trochanter down to the bone. The periosteum was divided and peeled off, being found remarkably

adherent. The trochanter, which appeared as a large globular mass of bone, was trephined, and the bone was found very soft and inflamed. Punctures were then made through the opening in the bone in four different directions with a drill. The wound was plugged with oiled lint, and the boy placed in the double interrupted splint. Temp. 98° . He passed a very good night afterwards.

June, 1876. The patient came to Guy's to report progress, and was seen by Mr. Bryant. He was quite well, although with a stiff hip-joint, and had not had any pain in the part since he left, eight months previously.

Case 4.—Expansion of the Great Trochanter from Ostitis; drilling of bone without benefit; trephining; cure.—(Reported by Mr. R. T. JONES.)—Wm. W—, a coalheaver, æt. 49, was admitted into Job Ward on October 18th, 1876. He was a married man, and the father of seven children alive and healthy, and of two who died young. In 1869 he had gastric fever, during which he had retention of urine, for which an instrument was passed. Since that time he noticed that his right hip became stiff after sitting for any period.

In May 1876 his hip became much worse, and, more or less, he has ever since been confined to his bed. He has often had a sensation as "if the joint jumped in and out."

For some weeks his hip has been swollen; this was the case when admitted, and there seemed to be a shortening of the right limb. He complained of there being as much pain in the knee as in the hip, though the latter was made worse on pressing upwards at the knee. Any attempt to move the hip-joint caused pain, but the head of the femur moved smoothly. The limb was somewhat adducted, and the great trochanter was clearly expanded. Firm pressure over this spot caused pain. He was very restless at night, and unable to sleep, and was ordered thirty grains of chloral, and the following day a mixture of iron and quinine.

20th. The patient had retention of urine again. A No. 7 catheter was easily passed, and a quantity of clear amber-coloured urine was drawn off, after which he had relief.

24th. Chloroform was administered, and an incision about three inches long was made over the great trochanter, and the bone, which was found to be vascular and soft, drilled in six places. There was no abscess, but there was much rigidity about the joint, even when under the influence of chloroform.

25th. He has not suffered from sickness. There was slight hemorrhage from the wound, which had been left to granulate. Temp. 99.6° ; pulse 66.

27th. The patient has experienced no relief from the operation, so chloroform was again given, and the great trochanter was

trephined through the incision made on the 24th, and a piece of bone about an inch in diameter removed. A cavity containing pus and lymph was exposed. Some cancellous bone, which was very soft from chronic osteitis, was also gouged out.

28th. He is much more comfortable than he has been for some time, having less pain in the hip, but he feels very sick. Temp. 100·6°; pulse 78.

Three months later the man was quite well, that is, he was free from all pain, and could walk well upon the affected limb. There was, however, but little movement in the hip-joint.

Case 5.—Ostitis of the Shaft of the Tibia; trephining the bone; relief of pain; subsequent necrosis; recovery.—James W., æt. 20, was admitted into Guy's Hospital on June 5th, 1860, under Mr. Bryant's care, with enlargement of the shaft of the right tibia from chronic inflammation of eight months' standing, following a blow.

The bone was much expanded, and the seat of a constant and at times intense pain.

On June 26th the operation of trephining was performed, and a central circular piece of bloodless, waxy-looking bone taken away, after which all pain ceased.

The shaft of the affected bone, however, at a later period became necrotic; on October 20th, 1862, the sequestrum was removed and the wound healed. The tibia on January 15th, 1863, had contracted to nearly its normal dimensions.

Remarks.—In this series of cases the effects of chronic inflammation of the articular ends and shafts of bones are well illustrated, together with the advantages which may be expected to follow surgical treatment.

In the first case (Case 2) the cure was complete, the disease having been arrested in its early stages by simply drilling the head of the tibia before it had involved the knee-joint.

In the second case (Case 3) the great trochanter was trephined and drilled, and the operation was followed by recovery, the disease undergoing in the bone a cure by resolution, and in the hip-joint by ankylosis, the affection of the joint having existed some time before the operation was performed.

In the third example (Case 4), the patient being middle-aged (49), the minor operation of drilling the bone was chosen as a tentative measure with the hope that it might have proved sufficient, but as it failed to give relief trephining was had recourse to, and with such success as to relieve pain and arrest the progress of the disease. The case subsequently went on to a recovery, though with a stiff joint.

In Case 5 the relief which the operation of trephining afforded was very striking, and if no other result followed this measure, it would be enough to justify its adoption. There

was, however, good reason to believe that by the operation, in addition to the relief of pain, the inflammatory action was checked, the disease limited, and the exfoliation of the sequestrum hastened.

TREPHINING, OR MAKING A FREE OPENING INTO BONE, THE SUBJECT OF SUPPURATION.—It unquestionably must be admitted as a fact that surgeons were in the habit of using the trephine in cases of suppuration in bone in the time of John Hunter; since that great surgeon told his class in 1787 that “the potential or actual cautery or the crown of the trephine are often necessary to be employed in order to get at the seat of an abscess in bone” (MS. Lectures, 1787), and spoke as if the operation at that time were recognised and often adopted. At the same time it must be equally admitted that the practice had fallen into desuetude, for in recent times the credit of introducing it has always been attributed to Sir B. Brodie, and we find the case by which he brought the subject before the profession in 1846 described by Sir. W. Fergusson in his college lectures, 1864-5, as one of the “beacon lights of surgery never to be forgotten.”

That the practice is good no surgeon now doubts, although it may be a question whether it is followed as freely as it might be. The cause of this neglect is probably due to the difficulty of diagnosis and the fear in the mind of the surgeon of not finding pus in the part perforated, most practitioners having still a kind of dread of opening a part in which pus may be suspected without finding it. This error, however, would disappear if the fact I have attempted to convey in these pages were recognised, and if it were generally admitted that the operation of drilling or trephining a bone, the seat of a chronic inflammation, was as beneficial as it is known to be when suppuration exists. To help forward and to illustrate this practice the following cases are recorded.

Case 6.—*Abscess in the Head of the Tibia following a blow, treated by a free opening into the bone; recovery.*—Robert J., æt. 43, a tin miner from Cornwall, was admitted into Guy's Hospital under Mr. Bryant's care on July 1st, 1869, with his left leg flexed upon the thigh, a discharging sinus on the inner border of the popliteal space, and marked expansion of the head of the left tibia.

From the history of the case it was elicited that he had had, twenty-five years previously, some disease of the head of the bone, and exfoliation of a sequestrum six months later, followed by recovery; that he had been well subsequently for twenty years, when he received a heavy blow on the inner side of the knee-joint, which was followed by swelling in the popliteal space and the formation of an abscess; that he was able in about a month to return to his work, although with some

inconvenience and occasional pain, and that for the last nine months the left leg had been gradually becoming bent upon the thigh. His general health during this time has been good.

On admission the leg was flexed at a right angle upon the thigh, and the head and upper third of the tibia were much enlarged. The skin over the bone was healthy, but about three inches from the head of the bone along its inner border there was a discharging opening; through this no bone could be felt. The head of the bone was the seat of constant pain, and at a spot about three inches from the joint firm pressure upon the shaft caused severe pain. By pressure over the popliteal space much pus could be made to escape.

The case was looked upon as one of abscess in the tibia, opening backwards, and discharging into the popliteal space. A free exploration of the bone was suggested.

On July 7th the following operation was performed:—A free incision was made down upon the bone over the seat of its expansion, and the soft parts with the periosteum pressed back. The expanded shell of bone was then perforated with a strong knife, and on a square inch of it being removed, a large cavity in the bone was exposed, which extended upwards towards the knee-joint. The cavity was lined with granulations, and contained pus, but no dead bone. It had evidently given way on its posterior surface, and had discharged itself towards the popliteal space. The limb was placed in a splint and raised, and the wound dressed with dry lint. Subsequently everything went on most favorably. The cavity in the bone filled up, all pain ceased, and by August 12th, that is, five weeks after the operation, the man was convalescent, and the wound had healed. The knee-joint was unaffected. A month later a complete cure was reported.

Case 7.—Abscess in the Head of the Tibia; opened by a scalpel; cure.—John R., æt. 29, a healthy man, came under my care at Guy's Hospital on March 4th, 1867, for severe pain in the head of his right tibia. It had come on gradually after a blow he had received upon the part four months previously. The pain was of a *dull, aching character*, and for three weeks had been *throbbing*. The head of the bone was expanded and rounded in front, and pressure upon it caused pain. The integument over the bone was apparently not involved, although to the hand it was hotter than that over the other. Fomentations and rest were ordered.

On March 11th the symptoms were no better, and it was clear that suppuration in the bone existed; a free incision was consequently made over the part with a scalpel, and on doing this a shell of bone was cut through, giving exit to about half

an ounce of pus. The relief was immediate. From this time steady convalescence followed, and by June 20th he was declared to be well.

Case 8.—Abscess in the Shaft of the Tibia; incision into the bone; recovery; old disease of the humerus, with resulting arrest of development.—James W., æt. 17, came under my care at Guy's Hospital on March 25th, 1867, with severe pain in the shaft of the right tibia. It had commenced without any previous injury two months before, and been followed after two or three weeks by swelling. This pain subsequently increased in severity, and the swelling continued to get larger. When seen the upper part of the shaft of the right tibia was much expanded, and the seat of a constant aching pain. Firm pressure over it caused great distress.

He was admitted into Guy's under my care on April 15th, as all his symptoms were increasing, and it was clear then that an abscess existed in the bone. A clean incision was accordingly made down upon the part *dividing the periosteum and a thin shell of bone, which was much expanded*; several drachms of thick pus escaped, leaving a well-defined cavity. The part rapidly healed after the operation, and all pains ceased; the boy left the hospital on May 23rd.

This boy had had bone removed from the right ramus of the lower jaw three years previously, and also from the shaft of the left humerus after so-called rheumatic fever. The left humerus measured $10\frac{1}{4}$ inches; right $12\frac{1}{4}$ inches.

Case 9.—Acute Abscess and Necrosis in the Head of the Tibia; incision into the bone; recovery, with a sound knee.—(Reported by Mr. REID.)—George H., æt. 15, a healthy lad, was admitted on July 5th, 1871, under Mr. Bryant's care, with some affection of his leg. It appears that a month before his admission he received a kick on the shin of his left leg which knocked him down; he felt, however, no ill-effects from it at the time. A week after he knocked his knee, or rather his leg, just below his knee, in getting into a cart. The blow gave him great pain, and he walked home with much difficulty. Has not been able to leave his bed since. The pain and the swelling increasing, he came to Guy's. Under advice he poulticed his leg after the accident, and, two days before, an abscess over the injured part was lanced, and much pus evacuated.

On admission.—The upper part of the left leg from the tuberosity of the tibia downwards is swollen and inflamed; and over the tuberosity a discharging sinus exists. The boy is in great pain. The leg was raised and fomentations were applied, but as the pain was very severe Mr. Bryant directed that a free incision should be made over the head of the tibia. This was done, and much pus evacuated thereby; immediate

relief followed. For three weeks everything went on well. His pain was much less than it had been ; the wound also nearly healed. A fistulous opening, however, remained, with sprouting granulations from the orifice, which Mr. Bryant said indicated diseased bone. None was, however, detected by a probe. A week later, however, Mr. Bryant thought it right to explore the part ; for, he said, if dead bone existed it ought to be removed, as it was in the head of the tibia, and consequently close to the knee-joint, which might become involved. This he did on August 1st, and exposed a cavity in the head of the tibia, the size of a walnut, lined with membrane ; in the centre of the cavity an irregularly round sequestrum was found. This was removed, and from that time all pain ceased, the wound rapidly granulated up, and the boy left August 30th, convalescent, with a perfectly sound knee-joint.

OPERATIVE INTERFERENCE IN ACUTE PERIOSTITIS AND ENDOSTITIS.—“The periosteal and endosteal membranes being intimately connected, inflammation originating in one is very liable to extend to the other ; and in proportion to the nature or amount of this extension will the inflammation be periosteal or endosteal, and the affection either periostitis or osteitis.” In acute inflammation of these tissues the two affections consequently must be clinically regarded together, since in both, pathologically, there is rapid effusion with extreme tension of tissue ; and clinically, deep swelling, severe local pain, and constitutional disturbance ; the general and local symptoms being frequently so severe as to raise the suspicion of the patient being the subject of an acute rheumatic rather than of an acute local affection.

The danger of leaving these cases to themselves, moreover, is known to all surgeons, for on the one hand blood poisoning is a common and rapid result of the disease, and on the other when this does not take place, more or less extensive necrosis of bone almost always ensues ; and if the disease be near a joint, it becomes rapidly disorganised.

It is, therefore, with the view of preventing these consequences that early operative interference is called for, and, I must add, that I know of no instances in which it is applied with greater benefit or happier result ; since, when the inflammation is periosteal, a free incision down to the bone not only relieves tension and consequently pain, but gives vent to inflammatory effusion, and tends to check the progress of the disease ; when endosteal, the same proceeding effects in a degree the same ends, and where it fails, tapping the bone by a drill or trephine may be had recourse to with the certainty of benefit. Indeed, a free incision down to bone in periostitis as well as in endostitis does nothing but good, and drilling or trephining

inflamed bone, even when it fails to cure, tends to check the progress of the disease, and relieves pain.

The cases which have been recorded, I trust, prove the points which I have wished to illustrate, and justify the following conclusions.

That in acute periostitis or endostitis, a free incision down to the bone, by relieving tension and giving exit to inflammatory effusion, does nothing but good; and that it should be made as early in the progress of the case as the diagnosis will justify, and if possible before pus has formed. The very commonly fatal termination of these cases by blood poisoning, when they are left to run their course unchecked, renders this measure imperative.

That in all forms of endostitis, or osteo-myelitis of long bones, in which more or less intense and persistent pain is a prominent symptom, the operation of drilling, trephining, or making a free opening into the bone should be entertained, as any one of these measures tends to check the progress of the disease, and in most cases relieves pain.

In flat bones, such as those of the head, and in cases in which the preceding measures seem too severe, the simpler operation of cutting down upon the bone and separating the periosteum from it should be performed.

That in all cases of *suspected* abscess in bone, the same operative proceedings should be carried out, the operation of trephining inflamed bone suspected to be the seat of suppuration being generally as successful in relieving pain and effecting a cure as it is well known to be when a local abscess in bone is found to exist.—*Guy's Hospital Reports*, 1879, p. 5.

29.—THE SETON IN CHRONIC PATELLAR BURSITIS.

By Dr. J. A. AUSTIN, Tongue, Lairg, Sutherlandshire.

There are two principal methods of treating the disease known as chronic patellar bursitis, or chronic housemaid's knee—viz., the *uncertain* and the *certain* method. Of the latter, one variety, I believe, consists in getting rid of the effusion in the bursal sac, and, by adopting measures for keeping the knee-joint extended, to prevent the influence of pressure, tension, or friction involved in the frequent act of flexion, which, I understand, are the great perpetuators of the mischief in these and similar cases.

There is, however, another variety of the certain method, which, though it may be said by some that it is neither so elegant nor convenient as the former, is yet as expeditious as any, and certainly cannot be surpassed for its certain and satisfactory results. I allude to the treatment by the seton.

To enter into a few details. The seton, composed of a double silk thread moistened in weak carbolic oil, is conveniently passed through the same cannula which draws off the fluid, and in certain circumstances, where the avoidance of pain is urgently required, the ether spray will be found a great boon. Too small a trocar is not to be selected, or the apertures will be apt to close up and obstruct the discharges. To have to enlarge them afterwards would be exceedingly disagreeable. A pad of lint, moistened also with carbolic oil, covered with gutta-percha tissue, and the whole secured by a few turns of a bandage, is both agreeable to the patient and helps to maintain the patency of the apertures. The seton should be drawn every morning, in order to present a fresh portion of it each time to the suppurating interior, and the pus encouraged to ooze out by frequent and gentle pressure with the fingers. In five or six days the seton may be withdrawn, and after five or six days more of rest the patient may be allowed to walk about. Should any congestion or weakness be left behind, it is effectually overcome by the local use of the cold douche.

Iodine, blistering, pressure, and even simple tapping, are very uncertain remedies, and their employment in nearly every case is merely a source of additional annoyance to both the patient and the practitioner.—*Lancet*, July 12, 1879, p. 44.

30.—NOTE ON A CASE OF LATERAL CURVATURE OF THE SPINE TREATED BY A NEW METHOD.

By GEORGE A. BERRY, Esq., M.B.

Cases of aggravated lateral curvature of the spine necessarily require prolonged treatment, and must remain long under observation before the final result of any treatment is definitely arrived at. This, it is hoped, may serve as an excuse for proposing the method adopted in the following case, which has not yet been brought to a conclusion, but which I am unable to retain any longer under observation, as the patient lives on a small island in the Western Highlands, and must shortly return home. The apparatus used, however, seems so thoroughly to fulfil the indications required, and the improvement produced has been so marked and rapid, and that after a fair trial of Sayre's method, that I have considered it worth while to publish the case, as a suggestion at any rate, notwithstanding its being necessarily incomplete, and the more so, that not being engaged in general practice, I shall not likely have any further opportunity of treating similar cases.

Robert Kirk, aged 9, a native of Eigge, was admitted into Ward 14, Surgical, under the care of Dr. Duncan, in December last. He was found to be the subject of lateral curvature of

the spine to an unusually exaggerated degree, the curvature being to the left. He could not stand erect, and walked with an extremely waddling gait. This condition, which had lasted for two years, followed, according to his own statement, immediately upon a prolonged attack of what he called rheumatism of the back and limbs, which confined him to bed for a year. The curvature was found greatly to disappear when the weight of the head and shoulders was removed by suspension or otherwise.

Two attempts having been made unsuccessfully at maintaining extension by means of Sayre's jacket applied whilst the patient was swung by the neck and arms, I obtained Dr. Duncan's permission to try the following treatment all the more readily, as he has been in the habit in his clinique of pointing out the inefficiency of Sayre's jacket alone in these cases, and had himself been considering the advisability of trying some similar modification of Sayre's treatment. He points out that, owing to the gradual slipping of the jacket, extension is not sufficiently maintained.

Two large pieces of extension plaster were applied over the whole of the back, on each side of the spine (straightened at the time by extension in the recumbent position), and reaching well round the sides to the front of the ribs. Having found that by pulling on the ends of the plaster the spine could be easily straightened, a plaster-of-Paris jacket was then applied according to Sayre's method, into which was incorporated a steel splint differing from the jury-mast used by that surgeon for the treatment of cervical caries, etc., in having a cross-bar at the level of the shoulders, which was capable of being lengthened by means of a rack and key, instead of the ordinary headpiece. After the plaster-jacket had set, the ends of the extension plaster were brought round the horizontal bar, and tightly sewn in position. On then screwing up the splint to a height which fell short of causing the patient any great inconvenience, extension was applied to the skin of the back, the counter extension being distributed over the pelvis through the jacket. The shoulders were at the same time drawn back to the cross-bar, and maintained in that position by means of a turn or two of a bandage.

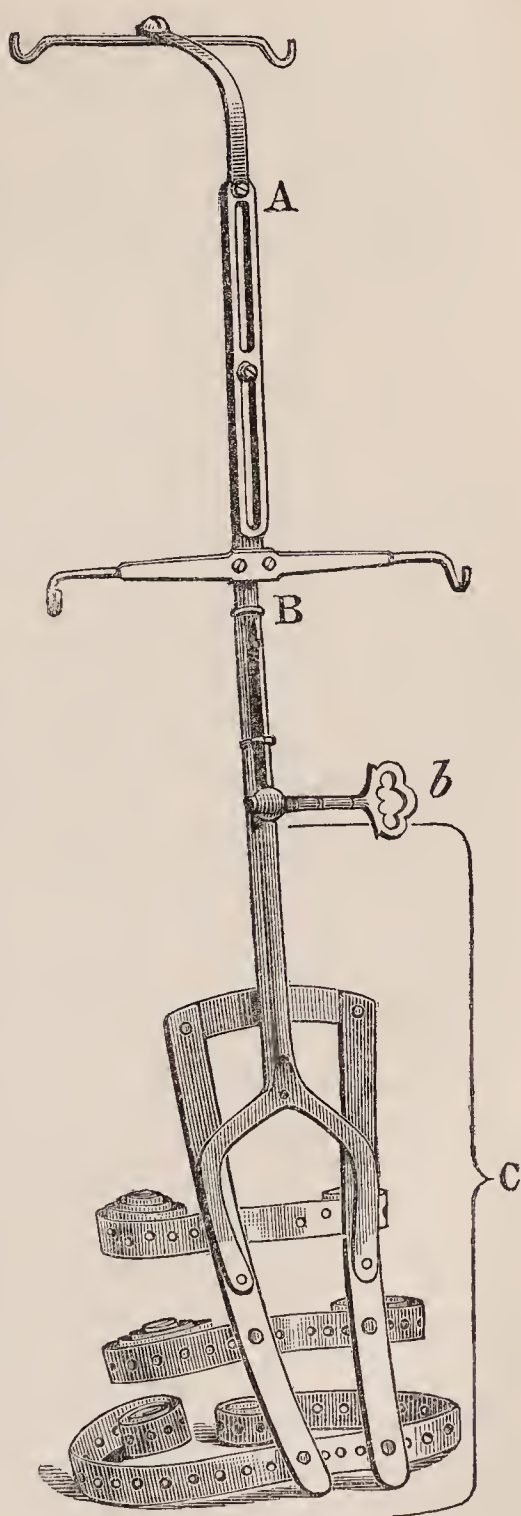
On removing this apparatus after four weeks, the patient could stand perfectly erect, and the spine exhibited only a trace of the previously existing curvature, and although the patient ran about as usual for three days only a slight trace of return could be detected. There was at the same time a very considerable improvement in his gait. The second application of the splint was not so successfully made, and had to be taken down at the end of three weeks. The result, however, was

still very good, although there can be little doubt that had it been better applied it would have been still better.

It may be well to state briefly what points seem to require attention in order to get the full effect of extension applied in this way. In the first place, it is most important that the direction of extension should lie in a plane parallel to, and as close as possible to the back, and extension should not begin until the horizontal portion of the splint is on a level with the shoulders, so that the whole of the force may be utilized, and the strapping not pulled from the skin, which was greatly the cause of the second application not being so successful in the case cited, as the plaster was detached down to the centre of the back, to below the upper curve, in fact. It is in order to avoid this separation of the plaster from the skin that it appears advisable to brace back the shoulders.

Again, the splint should be screwed up at least once a day, as it has a tendency to slacken from the plaster slipping slightly up, and the jacket slightly down, and perhaps also from a slight stretching of the skin.

The method of applying the extension strapping to the whole surface of the skin of the back and sides was suggested to me by Dr. Duncan, and is no doubt better than merely using two strips down each side of the spine, as I



DESCRIPTION OF DIAGRAM.

A, Head-piece, capable of being adapted in the usual way.

B, Transverse bar, round which strapping fastened, capable of being moved up and down by the key at *b*.

C, Portion below the key which is incorporated with plaster-of-Paris jacket.

had at first intended, especially if applied while the back is extended. Further, it is a good thing to swing the patient while turning the ends of the strapping round the cross-bar. The reason for this is obvious. Finally, it is probable that the addition to the splint of a jury-mast head-piece, in order to take off the weight of the head and neck, would still further enhance its efficacy. Such a splint has been made, and will be applied to the patient before he is discharged.—*Edinburgh Medical Journal*, July, 1879, p. 45.

31.—ON THE TREATMENT OF GENU VALGUM BY SIMPLE
INCISION AT THE LOWER END OF THE FEMUR
ANTISEPTICALLY DONE.

By Dr. WILLIAM MACEWEN, Surgeon to, and Lecturer on
Clinical Surgery, Royal Infirmary, Glasgow.

In a lecture recently published, I stated that the most suitable age for operating in knock-knee and bow-leg was from eight to about twenty years. On several occasions, I refused to operate on those above twenty. Since that time, however, as those of you who attend my wards have seen, four patients have been operated on by me over twenty years; two of them having knock-knee, one a bow-leg, and one an angular ankylosis of the knee-joint; their ages being twenty-two, twenty-six, twenty-seven, and thirty-two years respectively. In the first case, a double osteotomy, the temperature registered rather more than one degree above normal during the first two nights, but all the morning and all the subsequent evening temperatures were normal. In the three other cases, the temperature at no time rose above normal. The wounds were looked at for the first time two weeks after the operation, and were found quite superficial, the epithelium beginning to cover them; in three weeks, they were covered with epithelium. They complained of little or no pain. These results were highly satisfactory, and, as far as they go, tend to show that patients up to thirty-two years of age may be operated on with safety. The operator must, however, bear in mind that osteotomy in patients of this age is quite a different thing from the same operation in the adolescent. In the latter, the bones are much softer, and yield more readily; in the former, they are harder, and tend to split at right angles to the instrument, unless the manipulation is proceeded with carefully.

This leads to the consideration of the kind of instruments which ought to be employed, and the method of using them. It may be premised that some surgeons have expressed their belief that it is a matter of little moment what kind of chisel is used, provided it is strong enough; while others who practise the simple incision state that, as far as form is concerned, the

ordinary carpenter's chisel is quite suitable. I cannot agree with these opinions, believing, as I do, that the form and quality of the instrument are of very great moment. If one wishes to remove a wedge of bone, a chisel having the same form as the carpenter's would be suitable; the carpenter's chisel having one straight and one bevelled edge. In using such an instrument, the straight edge ought to be kept towards the surface which is to remain in the limb, the bevelled edge towards the part to be removed.

If this chisel were used to divide by simple incision the femoral shaft at the knee, where there may be one to three inches of thickness, there would be a decided tendency for it to take a wrong course, in spite of the efforts of the surgeon to keep it correct. Instead of going directly in, it would incline toward the straight edge, and the further it penetrated the greater would this inclination become. To attempt to correct this wrong course half way through the shaft, would be making matters worse. On one occasion, a gentleman, while operating for knock-knee, introduced a chisel with the straight edge downwards, which penetrated a short distance and then sloped downwards instead of going straight in. The instrument for some reason, was then removed and placed on the table. Afterwards it was picked up and reinserted; this time, however, with the straight edge upwards. After traversing the aperture first made, it began to cut a course for itself, and soon turned upwards; so that the incision, instead of being straight, was zig-zag. It is, perhaps, needless to say that such a result would be serious in any case; and, in order to obviate this tendency, another form of instrument ought to be used.

The form most suitable for the simple osseous division is one bevelled on both sides, so as to resemble a very slender wedge. Such an instrument passes easily into the bone, and maintains the direction given to it by the hand, having no tendency to glide either to one side or to the other. It is very easily withdrawn, and does not tend to become fixed in the same way as a chisel with a shoulder such as the carpenter's. If the bone be large and very dense, several chisels ought to be used. The first, being thick, makes the incision through the dense osseous surface on the inner side of the femur; the second, finer, is introduced into the groove made by the first, and comes into play directly on the deeper layers of the bone. A third may be used if necessary. On the other hand, in the adolescent, the bone may be found sufficiently yielding to permit the osteotomy to be completed by a single instrument; the aperture assuming the shape of the instrument, v-shaped, with the base inwards. In performing such operations, the instrument must be thoroughly trustworthy. From what I have been able to gather through

others, ordinary carpenter's chisels have been used by some surgeons, and they have been surprised that some of them have broken, leaving small pieces of steel in the interior of the bone. Now, the chisel ought to be made to suit the material upon which it is to act, not only in respect to its form, but also as to its temper and the "stuff" of which it is made. It ought to be made of very fine steel, tempered so as to prevent brittleness or too great softness. If it be brittle, it may leave a portion of metal in the tissues; if too soft, it will turn and curl up at the edge; of the two evils, the latter is the least.

I have had several sets of instruments manufactured for me, but the most trustworthy and satisfactory have been made by Mr. Macdonald, who put himself to some trouble to carry out my instructions concerning them, and also in determining the exact temper necessary. As the latter is a matter of considerable importance, the particulars arrived at may be mentioned in a couple of sentences. They were made from Stubbs' finest five-eighths steel, forged at a low heat; tempered by raising the instrument to a cherry red heat, dipping it into oil, and then plunging it into water until cold; next polished, and about an inch of the chisel, measuring from its cutting edge, having its temper drawn back, by raising it to a copper yellow colour with purple spots (probably a temperature of 520 deg. Fahr.) In these instruments, only a small portion of the chisel, embracing the cutting edge, is raised to a great degree of hardness; the remainder of the blade is comparatively soft, so as to avoid the possibility of snapping. Though this is applicable to all the chisels, it is particularly so to the finest, especially when used in cases where the bone is very thick and the instrument has to be deeply inserted. A good material for testing the edge of the instrument is the thick part of the hough-bone of an ox. If one finds that the instrument will neither turn nor chip in penetrating such a bone, it may be concluded that it is well suited for cutting any human bone. These instruments stood this test. I have found them to possess the quality of great hardness, combined with toughness, and hitherto, although used on some very hard and dense human bones, they have neither turned nor chipped. The handle and the blade form one piece. The handle is octagonal, and I prefer it to the oval or cylindrical, as it gives a better grip, and enables the operator to detect more readily any deviation which the instrument may assume. The top of the chisel is furnished with a rounded projecting head, against which the thumb of the operator rests as he steadies the instrument; it also serves as a support whereby the instrument may be gently levered out of the section. The figures 1 and 2 are placed near the head on the surfaces which correspond to the thickness of the blade, in order that

they may serve as a means of distinction when the blade is obscured in the tissues. The borders of the blade are marked with half inches, the figures being extremely light. The thickness of the bone can be ascertained in many cases, and thus the distance to be penetrated can be predetermined. The figures on the border of the instrument point out the depth to which it has penetrated, and thus serve as a guide. They are finely polished, not for appearance, but because the finer the surface the less opportunity will organic matter have of becoming adherent and afterwards decomposing.

Now, although there is nothing new in the principles on which this instrument is formed, yet its exact counterpart cannot be found out of surgery; and, though clearly of the chisel order, it has sufficient individuality to enable it to take a distinctive name. As the blade in shape resembles somewhat a transverse section of the blade of a pocket-knife, and as it cuts much in the same way, it might be described as a knife for cutting bone; or, to employ a single word which would at once distinguish it from the ordinary chisel and at the same time be more euphemistic to a patient's ear, it might be called an *osteotome*.

A fact often overlooked is the marked difference between the antero-posterior diameters of the outer and inner borders of the femur at the seat of operation. For a short distance above the condyles, the femur has a much thicker outer than inner border; in many instances, the outer is twice as thick as the inner. If the form of the bone be not borne in mind, the surgeon may think that he has divided it sufficiently, and yet he will find that it will not yield, owing, in most cases, to the posterior outer part remaining intact. The line of osseous incision is considerably above the epiphysis, and consequently cannot do it harm. The epiphysis may be surgically represented by a line drawn across the femur at the level of the highest point of the femoral articulating surfaces, and it runs through or just below the spine on the inner condyle for the insertion of the adductor magnus tendon; so that, in making the osteotomy an inch to an inch and a half above the spine, the epiphysis will be cleared. The only portion of the synovial pouch which is as high as the osseous incision is the narrow prolongation under the patellar ligament. This prolongation may reach in the adult about a couple of inches above the front part of the femoral articular surface. It is somewhat triangular in shape, its base being at the condyles, and it gradually tapers toward the middle line as it ascends. It is attached to the under surface of the ligament and moves with it. There is generally a quantity of adipose tissue between the pouch and the bone. The point selected by me for the incision in the soft parts is on the inner side of the limb posterior to the pouch. The manner of introducing the

osteotome, first longitudinally until it reaches the bone, then turning it transversely, would push the pouch aside, even if it were so much distended as to reach so far as the inner side, and the osteotomy would be performed behind it. Regarding the arteries, the femoral passes through the aperture in the adductor magnus, much above the seat of the operation, and with ordinary care cannot be injured in its position behind the femur. The osteotome, in cutting the posterior inner part of the bone, ought to be directed from behind forward, so as to cut away from the artery. The anastomotica magna runs close to and parallel with the adductor magnus; the superficial articular runs off below the line of my incision, so that neither are injured. In the great majority of cases, no ligatures are required. (In my last thirty cases, no ligature has been applied.)

Thus simple osteotomy at this part in no way affects the articulation of the knee, does not open the synovial pouch, and is clear of the epiphysis. The longitudinal incision in the soft parts passes through skin, cellular tissue, and a few fibres of the vastus internus, and by dividing these at one stroke a wound is made which, treated antiseptically, heals without the production of a single drop of pus.

There is a space nearer the joint between the vastus internus and the adductor magnus, through which the femur may be reached without passing through muscle. By dissecting down on this space and lifting aside the vastus internus, the bone might be sufficiently exposed to allow the osteotomy to be performed. The non-division of the muscle is supposed to be an advantage, but the gain would be counterbalanced by the fact that, in operating here, one vessel would at least require ligature, some dissection would be necessary, a larger wound would be made, and the tissues would be more disturbed by the separation of the muscle upwards. In fact, it has so many drawbacks, and there is so very little to be gained by its adoption, that I have not attempted it. Besides, the site of incision figured in the drawing leaves nothing to be desired, owing to its simplicity and the results obtained by operating in that situation.

In operating here, the scalpel is introduced at once to the bone and a longitudinal incision is made, a little longer than the breadth of the instrument. Through this wound, the osteotome is introduced longitudinally, until it comes into contact with the bone, when it is turned transversely. Its lower border—the one which is for the time posterior—is made to glide on the bone until it comes fairly behind, when the entire cutting edge of the osteotome is applied, and the instrument is made to pass in a direction from behind forward and toward the outer side. From this position, it is made to move forward along the inner border, until it comes to the anterior surface, when it is directed from before backward and toward the outer posterior angle of the

femur. In keeping on these lines, there is no fear of injuring the artery. In the adult, the hard exterior of the bone resists the entrance of the instrument at the outset, but several strokes cause it to penetrate this superficial dense portion, when the instrument will pass easily through the cancellated substance. After a little experience, the surgeon recognises by touch and by hearing when the osteotome meets the hard layer on the opposite side of the bone. If it be considered desirable to penetrate this outer dense part of the bone, in doing so, the osteotome ought to be grasped firmly in the left hand, the inner border of the hand resting on the limb, so as to check instantly any impetus which might be considered too great. When the instrument is to be altered in position, it ought not to be pulled out in the ordinary way, as it is then liable to be removed from the wound, as well as from the bone. Let the left hand, with its inner border resting on the limb, grasp the instrument, while the thumb is pressed under the ridge afforded by the rounded head, and gently lever the osteotome outwards by an extension movement of the thumb. In this way, the removal may be regulated with precision.

When the operator thinks that the bone has been sufficiently divided, the osteotome is laid aside, a sponge saturated in 1 to 40 carbolised watery solution is placed over the wound, the surgeon holding this in one hand, which he at the same time employs as a fulcrum, while the other hand grasps the limb lower down, and gives it a quick jerk inwards, when the bone will snap or bend, as the case may be. The elastic webbing, placed on the limb prior to the operation, to restrain the cutaneous bleeding, is now removed. As soon as the vascularity of the limb has been restored, a pad of gauze is placed over the wound, and the splint applied. The splint is a modification of the half box, the outer portion being carried up like a long splint to about the third or fourth rib, and it projects beyond the posterior splint at the foot. This projection is fixed in the clamp of a bed rest, and so the whole splint and limb are kept motionless and steady. The thigh and knee are fixed to the outside splint, while the lower limb and foot are thrown inwards by abundant padding. When the patient recovers from the anæsthesia, the first thing to be seen to is the sensation, circulation, and movement of the toes; and the surgeon ought not to rest satisfied until he sees that the patient is able to move his toes and foot freely. If the patient be not able to do so, the dressings must be removed, and the limb put up anew. The state of the toes must be watched during the first forty-eight hours carefully, and afterwards looked at from time to time. If the temperature of the body remain about normal, and if there be immunity from pain and absence of discharge, the limb ought not to be touched for a fortnight. At the end of

that time the dressings may be removed, when the wound will be found superficial, and partly covered with epithelium.

As to the position of the limb during the divisions of the femur in genu valgum, that may vary according to the taste of the surgeon. When the limb is extended, the bulk of the vastus internus is less at the seat of operation than when the limb is thrown across the opposite leg; so that fewer fibres are divided when the incision is made when the limb is extended. On the other hand, when the limb to be operated on is turned on its outer side, and the leg flexed and placed over its neighbour, so that the inner side of the femur is turned uppermost, it gives on the whole a firmer position, and one more suitable, perhaps, for the action of the osteotome. At first, blocks of wood were placed under the limb to steady it, and to afford an unyielding surface against which the osteotome would act with greater effect. Some of these were convex, some were scooped out to suit the limb. One of my clinical students (Mr. Beattie) recommended the use of sand pillows, which I now employ, finding them much more suitable than wooden blocks. They may be moulded to the limb or particular part, and they afford a much more even, solid, and steady grip. They are damped before being used, as in this state the escape of fine dust is prevented, and the particles have greater cohesion. One about a foot square I find very serviceable. As a rule, I now operate with the limb in the extended position.

Before concluding, a fact may be referred to, representing the pathology of knock-knee as it occurred in the patient on whom, a short time ago, you saw me operating, and whose photograph I now show you. The general pathology of knock-knee is not intended to be discussed here, but merely the mention of a fact. Those of you who attend my wards were shown that, in this patient, the lower third of the femur was curved markedly inwards. This being so, the plane of the condyles was necessarily altered, so as to throw the internal condyle on a lower level than the external. Now this corresponds to what the French school believes to be the true pathology of knock-knee. But besides this, as far as could be ascertained in the living body, there was a quarter of an inch of increase beyond the normal length of the internal condyle; this increase in the length of the condyle itself being probably secondary.—*British Medical Journal*, May 3, 1879, p. 656.

32.—ON THE TREATMENT OF CONGENITAL TALIPES EQUINO-VARUS AFTER THE PERIOD OF INFANCY.

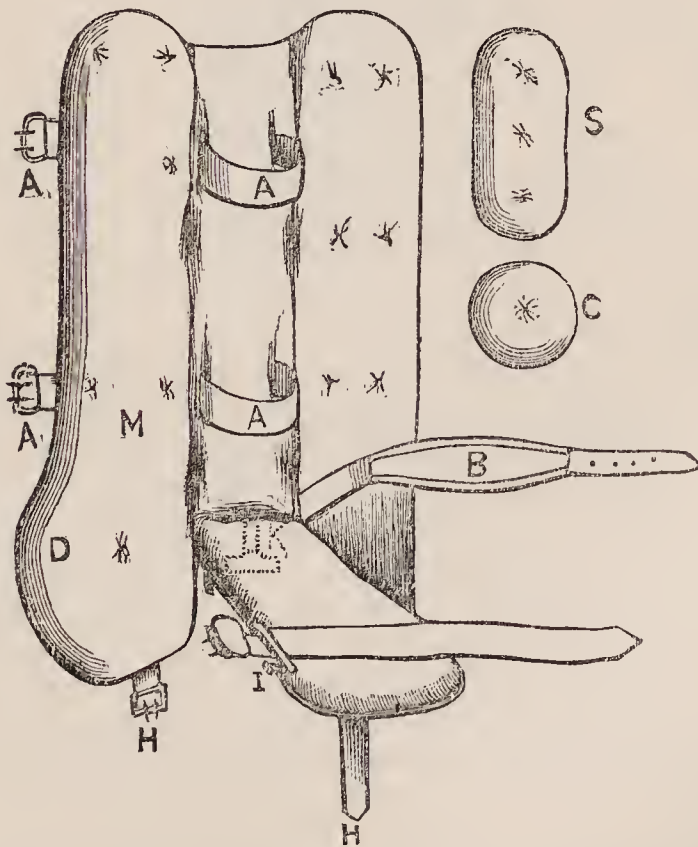
By HENRY F. BAKER, Esq., F.R.C.S. Edin., late House-Surgeon to St. Bartholomew's Hospital.

The treatment of congenital talipes equino-varus, after the period of infancy, may be conveniently divided into three parts

—1st. The cure of the simple infantile varus occasioned only by the contraction of the tibialis posticus, flexor longus digitorum, and the tibialis anticus. 2nd. The cure of the remaining acquired varus, caused by the contraction of the abductor pollicis, plantar fascia, and the various ligaments connected with the misshapen bones of the tarsus. 3rd. The equinus caused by the contracted tendo Achillis.

With regard to the treatment of the first part, after the successful division of the tendons mentioned above, the best instrument is, undoubtedly, a well-padded outside splint, to which the foot is gradually drawn by means of an ordinary bandage. When as great a part as possible of the varus has been removed by these simple means, the second part will still remain; and this is, of all, the most difficult to successfully overcome, even after the complete division of the abductor pollicis and the plantar fascia, chiefly on account of the great danger of producing sloughs by such an amount of pressure as will suffice to correct the deformity. The common Scarpa's shoe, useful as it is in some forms of talipes, is quite useless

FIG. 1.



here. Scarpa, indeed, invented a special instrument for the purpose, but it is almost impossible to use it without causing sloughs. In more modern times, also, many very clever instru-

ments have been devised with the same object in view, but in practice they have proved only partially successful.

For the efficient application of force in overcoming the resistance formed by the contracted tissues on the concave aspect of the deformed tarsus, the fulcrum must, I believe, be placed on some part of its convexity, and as near as possible to its most projecting point; the internal tubercle of the os calcis being made a fixed point, and the power being represented by the tension of a strap placed round the ball of the great toe. The problem therefore to be solved is—How can the fulcrum be applied in this position for the required length of time without causing sloughs?—and it appears to me that the only method is by rendering it a *movable* one. In other words, the pad which forms the fulcrum, instead of being a fixed part of the splint, should be unconnected with it, and thus made capable of being shifted from point to point along the outer edge of the tarsus, as circumstances require. The instrument which I have devised for carrying this into execution is figured above (Fig. 1). The figure represents the instrument open, and it will be seen to consist of a back-splint, connected with a foot-piece by means of a cog-wheel, K, at the heel, and having hinged to it two wings which extend well below the foot-piece, and have a buckle and strap, H, attached to their lower ends. A A are two webbing straps introduced through slots, to fix the leg firmly. B is a leather ankle-strap, wide and padded in the centre, to fix the heel immovably in its position; it is attached to a buckle at the back. D is a plate attached to the external wing; and C is the pad, or movable fulcrum, to be placed beneath it, and to press on the projection at the outer side of the foot. I is a rigid bar for carrying the toe-strap.

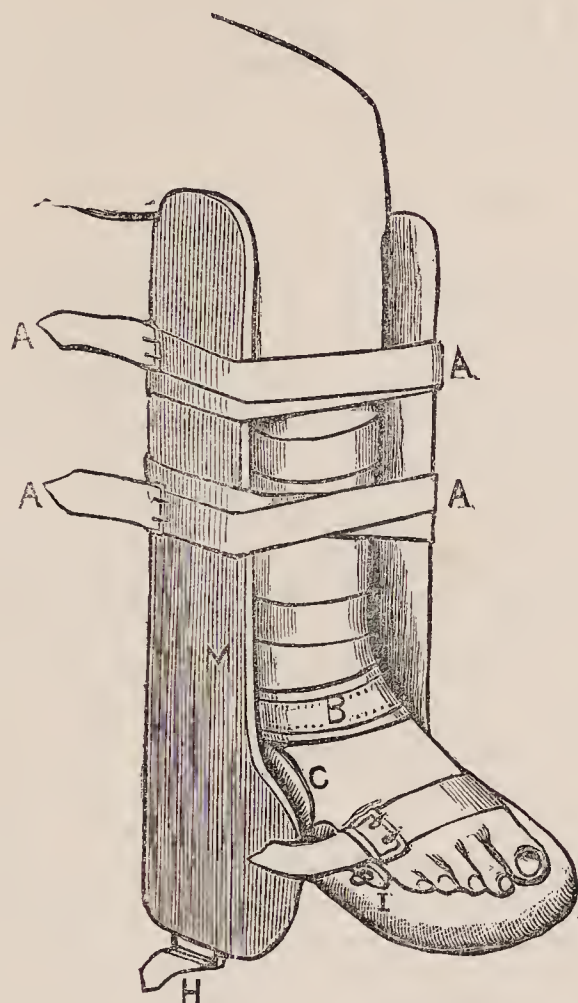
Fig. 2. represents the instrument in action. The leg is firmly held to the back-splint by the straps A A. The heel is kept in place by strap B, and the movable pad C is adjusted. The wings are brought together, and firmly fixed by the further action of the straps A A, and strap H; and the toe-strap is then tightened, and the cog-wheel, K, screwed up. By simply loosening the toe-strap, the pressure is at once relieved if this should be found necessary, as is frequently the case at night, when the foot becomes hot; and in a day or two, the wings being thrown back, *but the foot not being disturbed*, the pad, C, may be slightly shifted.

In some cases it will be found convenient to introduce the rectangular pad, s (Fig. 1), within the external wing at M, and so place the fulcrum on the outside of the leg; but this materially diminishes the power of the instrument.

This splint is equally well adapted for carrying out the third stage of the treatment—viz., correcting the equinus after the division of the tendo Achillis; and in some cases it can be used

for this purpose before the second period of the cure of the varus is quite completed.

FIG. 2.



A varus splint, constructed in some respects like the one described, was devised by the late Mr. Tamplin, and has been in use at the Royal Orthopædic Hospital for many years in the treatment of *infantile* varus, for which it is well adapted. It fails, however, altogether in the treatment of those more difficult cases of acquired tarsal deformity which are the subject of the present communication. The special points in which the splint here suggested differs from Mr. Tamplin's are—(1) the addition of an inner wing; (2) the projection of both wings well below the foot-plate, with connecting strap; (3) the rigid toe-bar attached to the sole-plate; (4) the leather padded ankle-strap; and (5)—the most important of all—the movable pad which forms the fulcrum.

If it should be thought advisable to combine the use of a spring or elastic pressure, this can be at once done by placing a steel spring in the position of the rigid toe-bar, or an elastic

toe-strap in the place of the one made of common webbing. Neither of these alterations, however, would be, in my opinion, an improvement.

I may mention that the instrument has been made for me by Mr. Gumpel, of Leicester-square; and I take this opportunity of acknowledging the very efficient manner in which my suggestions have been carried out.—*Lancet*, May 3, 1879, p. 626.

ORGANS OF CIRCULATION.

33.—ON FORCIPRESSURE AND THE USE OF PRESSURE-FORCEPS IN SURGERY.

By T. SPENCER WELLS, Esq., F.R.C.S., Surgeon to the Queen's Household; Consulting Surgeon to the Samaritan Hospital.

In the British Medical Journal of January 10, 1874, among valuable reports from metropolitan hospitals on the use of torsion in surgical operations, the following passage may be found:—“Mr. Spencer Wells (Samaritan Hospital) has introduced a kind of artery and torsion forceps, which very conveniently replace the old spring artery-forceps of Liston, and the bull-dogs used for *the temporary stoppage of bleeding vessels during operations*, while they are the most readily applied of any of the varieties of torsion-forceps met with in the shops. They were first made for Mr. Wells by Krohne and Sesseman. The grasping and holding extremity is roughened by rather deeply cut transverse teeth so that *the bleeding vessel is forcibly compressed, and its coats squeezed or almost crushed together*. This is alone often sufficient to stop the bleeding without any torsion, especially if the instrument be left on the vessel for a minute or more. But if the vessel be large, then two or more rotations may be added. Instead of the spring-catch, the fastening is effected by a Mathieu's catch in the handles. This is quite as easily fixed and opened as the spring, and is much less likely to get out of order. The instrument is made of steel, but is coated with nickel, which prevents any rusting after use. Mr. Spencer Wells, as is well known, trusts to the clamp for securing the pedicle in ovariectomy whenever it can be applied without too much pull on the uterus or broad ligament. Where the pedicle is very short or broad, he either uses the cautery, the ligature, or the *écraseur*. He has never trusted to torsion alone, though occasionally securing one or more vessels in this way. With the bleeding vessels in separated omentum, he trusts to torsion when the vessels are distinct; but when oozing surfaces rather than separate vessels are seen, he uses ligatures of fine pure silk, cutting off the ends short and returning them. He prefers this

silk to catgut. With bleeding vessels in the abdominal wall, torsion, or *simple compression with the torsion-forceps, is sufficient.*"

This passage proves that before 1874 I had employed forcipressure, not only for the *temporary* arrest of bleeding during surgical operations, but had designed forceps for the express purpose of so squeezing or crushing the coats of the bleeding vessel together, as *permanently* to stop bleeding from vessels of moderate size. I draw attention to the date of this paragraph because, in 1875, MM. Deny and Exchaquet, in a monograph entitled *De la Forcippressure, ou de l'Application des Pinces à l'Hémostasie Chirurgicale*, while admitting that many surgeons have used spring forceps of various forms for the *temporary* suppression of bleeding during operations, claim for M. Péan the merit of introducing forcipressure into surgical practice as a mode of permanently closing open vessels—the "definitive obliteration of vascular orifices by simple compression," not "simple pressure, but rather a sort of constriction, which the instrument employed by M. Péan effects upon the coats of the vessel." This claim in favour of M. Péan was very soon called in question by M. Kœberlé who published in 1877, a very interesting work, entitled "*De l'Hémostase Définitive par Compression Excessive.*" He speaks of pressure forceps as "*mes pinces hémostatiques,*" acting by strong compression so as to dry up—*dessêcher*—the compressed tissues, and rapidly effecting permanent stoppage of bleeding *hémostase définitive*—thus almost entirely suppressing the use of ligatures. He says that, before 1867, this use of forcipressure had never been utilised as a rapid method of permanently closing bleeding vessels—"comme méthode rapide d'hémostase définitive." He asserts that, in 1867, he *inaugurated* this "*méthode d'hémostase par compression excessive,*" that the use of his forceps dates from 1865, but that he had not used it until 1867, except as a means of temporary compression before applying ligatures.

I do not wish to enter into this discussion between MM. Kœberlé and Péan, nor to prove that I had preceded both of them by many years in the use of forcipressure. My chief object is to bring more prominently before the profession the many and great uses of this simple and rapid mode of stopping bleeding, and describing what I believe to be the best form of pressure-forceps hitherto constructed.

I can hardly recollect when I first began to use forceps instead of the fingers of an assistant for temporarily stopping bleeding during operations; but I believe I learned it from Mr. Bowman before he left King's College. I had often admired in the private and hospital practice of Sir William Fergusson the ready way in which Mr. Henry Smith and the late Mr. Price would instantly stop a spouting vessel by the finger, and tie it at a

glance from the great operator. But I well remember seeing Mr. Bowman extirpate a very large tumour from the neck, and quietly put a "bull-dog" upon every considerable vessel as he divided it. This must have been in 1854; because when I went to the Crimea in 1855, I took a number of "bull-dogs" with me; and after my return in 1856, I never went to any serious operation without several of different sizes. I used them in all my earlier cases of ovariectomy (beginning in 1858) for stopping any vessel which bled in the abdominal wall divided in the first incision. Referring to the fourth edition of Fergusson's Practical Surgery, published in 1857, I find at page 42 a drawing of these "bull-dogs," and an allusion to a plan which I used sometimes to adopt of putting a ligature on and tying with only one knot so as to crush the vessel, and then remove the ligature. This crushing of a bleeding vessel I had practised not only from the well-known lesson of torn arteries not bleeding like those cleanly-divided, and from the *écraseur*, but from what I had heard of the experiments of the late Mr. Webber, formerly of Yarmouth and Norwich. This surgeon about 1856 to 1858, made many efforts to introduce into practice in the metropolitan hospitals what he called "anti-ligature forceps." Mr. Ferguson, of Giltspur-street, who made the instruments, has sent me four specimens of the different kind of teeth used in the forceps of Mr. Webber.

The only published account of these forceps I have been able to find is in the Medical Circular in 1858 and 1859, Mr. Webber there stating that he had used them for more than four years. Mr. Ferguson does not remember when he first made the instruments. I only allude to them now as a proof that the use of forceps as a means of *permanently* suppressing hemorrhage had occupied the attention of English surgeons long before M. Kœberlé's inauguration in 1867. I cannot remember precisely when I began to find that the "bull-dogs," used at first only as a means of temporary compression, were sufficient to permanently close vessels of moderate size; but, in 1863, I began to increase the size of the "bull-dogs," and to attach long pieces of silver or iron wire to them, so that when used on omental vessels, or on bleeding vessels from torn adhesions on the inner surface of the abdominal wall, they should not be forgotten or lost. Then, as torsion came into more general use, and various forms of torsion-forceps were contrived, I arrived at the form of instruments described above in the reprint of the article published in 1874. Mr. Krohne tells me that he made the first of these forceps for me early in 1872.

The instruments of Pean, and of Kœberlé differ in the "catch," or mode of fixing the handles together; and in the joint—Kœberlé's being hinged, while Péan's blades can be

separated for easier and more complete cleansing. Péan has a great number made straight, of different lengths, and others curved or angular. But they all, like Kœberlé's, have the great disadvantage of an open space between the blades, which admits of entanglement of one instrument with another, or of the passage of omentum or other structures. This was a fault in my own earlier instruments. It has been completely corrected in the later instruments made for me by Mr. Hawksley, without at all lessening the compressing power exerted on the vessel. In October, 1878, Mr. Hawksley carefully tested the compressing power of different forceps when opened by a piece of leather one *millimètre* thick between the jaws of the forceps, and covering about four teeth from the points. The following table gives the result, as well as the force required to be exerted by the hand in closing the handles or fastening the catch or catches under each condition.

Pounds avoirdupois exerted by four teeth of the end of forceps when one millimètre apart.

Forceps.	First catch.	Second catch.
Kœberlé	—	$3\frac{1}{4}$
Péan	8	12
S. Wells (old)	$22\frac{1}{2}$	—
Ditto (new)	12	$22\frac{1}{2}$

It may be seen that in my old instrument there is only one catch. And in my new one, the second catch only exerts the same power as the first catch of the old instrument. But this is six or seven times greater than the second catch in Kœberlé's—and nearly double that of Péan's. When only the first catch in Kœberlé's instrument is closed, the points are separated about half a *centimètre*, so that they only compress anything more than that thickness. I have used all these instruments, but find them much less handy than my own, in which the handles meet without leaving any opening between them. The rings do not admit the thumb and finger too far; and the end which compresses the vessel is so bevelled, that, if it be desirable to apply a ligature, the silk will easily slip over the forceps, and not tie them together. Thus my instrument is not only useful in forcipressure and in torsion, but enables the surgeon to dispense with any other kind of artery-forceps, if he wish to apply a ligature.

The use of forcipressure in surgery may be considered under three heads: first, as one way of carrying out the principle of Esmarch's "bloodless surgery" during an operation; secondly, as a mode of dispensing with either the cautery or the *écraseur* as alternatives for knife or scissors; and, thirdly, as a substitute both for torsion and the ligature for permanently closing bleeding vessels.

Few can estimate more highly than I do the great advantages gained by the use of Esmarch's elastic bands and bandages in amputations, excision of joints, or in any operation where they can be conveniently applied. But in the operations which of late years I have been most frequently called upon to perform—the removal of large ovarian and uterine tumours, and amputation of the breast, the cure of ruptured perineum and vaginal fistulæ, or the removal of large tumours in various regions—Esmarch's proceedings are quite inapplicable. I have occasionally had Lister's tourniquet for compressing the aorta applied as a precautionary measure; but have only twice had to exert compression. In every other case, pressure-forceps have always been sufficient for the suppression of hemorrhage, at least until the completion of the operation. Any vessel divided in the first incision through the skin and integuments, either of the abdominal wall or any other part of the body, may be seized almost as soon as divided, and the forceps left hanging on as long as necessary. Deeper vessels may be secured in the same way. I have removed very large mammary tumours, one weighing nine, and another thirteen pounds; sometimes putting the forceps on a large vessel before dividing it, sometimes directly after the division, until from twelve to twenty forceps had been used, and thus large vascular tumours were separated with scarcely any loss of blood. In ovariectomy, shreds of vascular omentum or mesentery may be thus secured before separating them from the cyst, or any vessels which bleed after separation from the cyst may be made safe until the pedicle has been secured and the tumour removed. If it be inconvenient to tie the pedicle before dividing it, then two or three pressure-forceps may be applied, one at each extremity of the pedicle, and one or two more towards the middle; and the tumour may be cut away without any fear of losing the end of the pedicle or its vessels. In Cæsarean section, splenectomy, nephrotomy, and the removal of a large undescended testicle in a man, forcipressure has proved quite as useful to me as in ovariectomy or hysterectomy. In operating for the cure of old ruptures of the perineum, troublesome bleeding often occurs as the mucous membrane is denuded. If ligatures be used, they may interfere with union between the raw surfaces when they are brought together by suture; but, as a rule, pressure-forceps amply suffice for stopping all bleeding until the sutures have been passed and are ready for closing. A little pressure is then all that is needed. So in operating on vaginal fistulæ, very troublesome bleeding, which would interfere with accurate paring of the edges and passing of the sutures may be immediately and effectually controlled; and if the forceps be left on until it is time to fasten the sutures, it will almost always be found that bleeding has ceased.

I need now say very little as to the use of the *écraseur* or the cautery. There are cases where one or the other may be preferred to the knife or scissors. But, with a sufficient number of pressure-forceps at hand, the fear of hemorrhage need not influence the decision.

The permanent closure of vessels divided during an operation, or by an accident, is a question of greater importance than the mere temporary stopping of bleeding. Acupressure, filopressure, and uncipressure have all failed to gain general acceptance by practical surgeons. Torsion, though deserving of far more general adoption than it has hitherto attained, has made but very slow progress. But I have never yet found anyone who has tried forcipressure who has not been immediately convinced of its convenience and the great advantages obtained, at least in the temporary suppression of hemorrhage. Confidence in forcipressure as a substitute for the ligature, or as a mode of permanently closing considerable arteries, is attained more slowly, and requires much more observation and more extended trials before its real value can be accurately estimated. On this point, rather than rely on my own trials, I prefer giving a short *résumé* of the experience of Kœberlé, Péan, and Verneuil.

M. Verneuil, in 1875, published several very interesting papers on Forcipressure in the *Bulletins et Memoires de la Société de Chirurgie de Paris*. He explained how he had been led, in several cases where ordinary methods were inapplicable, simply to leave in the wound the forceps which he had used to seize the wounded vessel. His first case, about 1865, was an uterine polypus with a long pedicle. He cut the pedicle, and was surprised by free bleeding from a considerable artery. Cold injections proving useless, he seized the end of the pedicle by his polypus-forceps, tied the handles together, and left them in for two days without inconvenience to the patient. In 1869, he treated an artery deep in the palm of the hand in a similar manner, leaving the forceps for five days. In 1870, a wound of the internal mammary artery was also successfully treated, the forceps being removed after forty-eight hours. In 1873, in removing the coccyx, the lateral sacral artery was wounded and secured by forceps, which were left in for four days. In 1874, a very vascular nasopharyngeal polypus was removed by a wire *écraseur*; and, very free bleeding not being stopped by plugging, the bleeding surface was seized by forceps, which were left in till the next morning and then removed without any recurrence of bleeding. Equally successful cases of wounds of the radial artery, of bleeding from the tonsils after removal, and from the interosseal artery after trephining, are also related. A large nævus was surrounded by ten forceps (which were left on till they fell off on the fifth and seventh days), and the growth was thus removed without the

loss of one drop of blood. The remarks of M. Verneuil on these cases, on the varieties of forceps successively constructed, on the use of compressors of arteries by Assalini and others for the cure of aneurism, on the movable forceps of Nunneley and the "canula artery-forceps" of Wolfe of Aberdeen (described in the *British Medical Journal* in 1867), the "self-acting wire compress" of Taylor (described in the same *Journal* in 1868), and Richardson's tubular compress (in the *Medical Times* in 1869)—are all worthy of attentive perusal. He then analyses the results of forcipressure in 27 cases of forcipressure of large vessels—1 axillary, 1 subclavian, 17 femoral, 2 tibial, 2 brachial, 3 of the forearm, and 1 radial in the hand. In 22 of these cases, the success was complete. The forceps were left on for various periods from twenty-four hours to the fifth or sixth day, and four times until they came away spontaneously. In only one of the 27 cases was there secondary hemorrhage. The inconvenience to the patient and the interference with the healing of the wound has been greatly exaggerated.

As a means of preventing or anticipating hemorrhage, M. Verneuil also shows how forcipressure becomes useful before paring the edges of hare-lip, or removing epithelial growths off the lip or cheek, or portions of the tongue, in compressing the spermatic cord before castration, and in removing growths from the neck of the uterus.

The concluding portion of M. Verneuil's papers, on multiple and prolonged forcipressure as a mode of permanently stopping bleeding, is perhaps the most important of all. He shows that M. Péan has carried out this practice far beyond other surgeons. In comparing the relative advantages and disadvantages of leaving several forceps or several ligatures in a wound, it is thought that both are equally efficacious if the forceps be left on long enough. In rapidity of application, the advantage is altogether on the side of forcipressure. The sponging and irritation of the wound necessary for the ligature are spared, and the pain after recovering from anæsthesia is said to be less, if forceps are properly supported, than from ligatures. In quiet patients, forceps may be left in the mouth, vagina, or rectum; but with timid or restless persons the ligature should be preferred. But the main advantage of forcipressure is that the forceps need not be left on more than a day or two, perhaps not many hours; whereas ligatures separate slowly, and if not cut short retard the healing of wounds.

In the account of M. Péan's practice, given by Messrs. Deny and Exchaquet, a number of general observations on the different modes of suppressing hemorrhage are followed by examples of the use of forcipressure, *before* operating, as a means of *preventing* hemorrhage; and particular forms of forceps are

described for the tongue, lips, and neck of the uterus. Then a number of cases are related, where forcipressure was not only of temporary utility, but as permanently stopping bleeding, "*moyen d'hémostasie définitive*":—amputation of the breast and limbs; resection of joints; operations on the scalp, mouth, tongue, neck, trachea, and rectum; castration, and gastrotomy; also on vessels not divided, as in aneurism and varices; the general conclusion being that the forceps may be removed in from two to thirty-six hours after the operation.

In Kœberlé's work the same order is followed. Into the question of priority at issue with M. Péan I need not enter; nor into the difference between the instruments of Paris and Strassburg, as I think those of London far superior to both. But I join heartily in the concluding remarks of Kœberlé, to the effect that forcipressure is easy and rapid, enabling the surgeon to do without assistants—thus remarkably simplifying the practice of operative surgery. When divided vessels are small, the pressure exerted during the operation is quite long enough. When vessels are large, the forceps may be left in for some hours, for a day or more. Thus ligatures may be entirely suppressed, except in some few cases where forceps cannot be left; even then, they facilitate the application of ligatures. After the removal of the forceps, which may be looked upon as a ligature or compressor removable at will, no foreign body remains in the wound.

My own experience fully bears out the conclusions of the French surgeons; and I feel certain that, if good forceps are used, forcipressure will soon become very general in the practice of British Surgeons.—*British Medical Journal*, June 21 and July 5, 1879, pp. 926, 3.

34.—NOTE ON STITCHES OF RELAXATION.

By Dr. J. C. OGILVIE WILL, Surgeon to the Aberdeen Royal Infirmary.

In one of Professor Lister's papers, "On Recent Improvements in the Details of Antiseptic Surgery," which appeared in 1875, he alluded to the utility of button-sutures, a name which he applied to a form of stitches of relaxation. His observations are doubtless familiar to all surgeons; it is therefore needless to review them, so I shall content myself with the statement that I can, from a not inconsiderable experience of button-sutures, bear testimony to the advantages accruing from their use; and the object of the present communication will be amply fulfilled if I succeed in furthering the employment of this mode of keeping cut parts together by the description of a modification of the button which I had made for me soon after

the appearance of the paper referred to, by which the use of the appliance is rendered easier, and which, I think, removes the only difficulty connected with the manipulation of the buttons originally suggested.

Professor Lister's stitch of relaxation is thus described by him. "It consists of two oval pieces of sheet-lead, about one-twentieth of an inch thick, with a central perforation to receive a moderately thick silver wire. The silver wire is first passed as an ordinary suture, except that it is carried at an unusually great distance from the edge of the wound, both as regards surface and depth; each end of the wire is then passed through the hole in the corresponding lead button, and secured by being wound once round the shorter diameter. The two buttons thus take the place of the tips of two fingers of the two hands in giving support to the deeper parts of the wound, while leaving the cutaneous margins free; and, when the wound is at all extensive, several pairs of buttons are applied in this way, constituting a sort of interrupted quilled suture." If it be necessary to relax the wire, this is readily done "by unwinding the wire from one of the buttons, and after straightening it, allowing the button to slip upon it to any degree to which the tension disposes it, and then fixing it again by winding the wire round it." The difficulty to which I have alluded as attending the use of the buttons recommended by Professor Lister consists in twisting the wire round the disc of lead so accurately that it will maintain its position, this being materially increased by the slippery condition of the operator's hands at the conclusion of an operation, or by the benumbed condition of the fingers induced by prolonged exposure to the influence of carbolic spray. This difficulty I experienced when first employing button-sutures, and frequently since then when using them; but the mode of fastening the wires now proposed disposes of this obstacle most effectually, and thus removes the only possible objection to the employment of stitches of relaxation.

The button which I now use is exactly similar in shape and size to Professor Lister's, but a little knobbed stem springs from each side of the central aperture, round which the wire is twisted in figure-of-eight fashion; thus the wire can be drawn well home, is kept in exact position, and can be relaxed at will, and reapplied with the greatest ease. If chromic catgut be employed in place of wire, a thread long enough to allow subsequent relaxation and retying should be used, the superfluous catgut being twisted round and round the little pins. The buttons are made of silver, and are therefore comparatively costly; but were any demand to arise for them, the makers (Jamieson and Son, Union Street, Aberdeen) would

make them of plated material, by which their price would be rendered extremely trifling. Being pliable, the button can be bent so as to accommodate itself to any surface, while the material of which it is composed renders its thorough cleansing both easy and certain.

The largest and most generally useful of three sizes of the button which I have by me measures three-fourths of an inch in length by half an inch in breadth. In using the smaller ones, they should be kept flat, while the wire is being twisted round the pins, by pressure with the finger-nail on the point of a sharp-pointed scissors. Any enumeration of the conditions where the use of this retentive appliance is indicated is uncalled for, as they must be sufficiently apparent to all; and as the paper quoted is in an eminent degree suggestive, it only remains for me to direct attention to its utility in those cases where amputation by a rectangular or other long flap is performed. Possessing, as I do, a strong preference for the operation originally proposed by the late Mr. Teale, I resort to his method of amputation in every case where I can, without any undue sacrifice of parts, put it in force; and in these cases, almost more than any others, I have seen the usefulness of button-sutures. The use of plasters is hardly compatible with that of the antiseptic method; therefore some other means are frequently necessary for the purpose of supporting the long and somewhat unwieldy flap resulting from Teale's amputation; and even in those cases where antiseptic treatment is not pursued, plasters, although undoubtedly useful in keeping the cut parts in accurate apposition, are open to many objections; but in button-sutures we have a most efficient and unobjectionable substitute for plasters, and one which, so far as keeping the cut parts together, leaves nothing to be desired. In conclusion, I would say that I feel satisfied that a single trial of the stitches of relaxation recommended will prove sufficient to show their value, and that their continued use will lead all surgeons to agree with me in regarding them as not the least important of Professor Lister's many contributions to practical surgery.—*British Medical Journal*, June 21, 1879, p. 931.

35.—POPLITEAL ANEURISM CURED BY APPLICATION OF ESMARCH'S BANDAGE AND DIGITAL COMPRESSION.

Under the care of Mr. HEWETSON, at the York County Hospital.

It will be noticed in the following case, for the notes of which we are indebted to Mr. F. H. Weekes, house-surgeon, that the application of the elastic bandage for eighty minutes effected a great change in the character of the aneurism,

although consolidation was scarcely sufficient to justify a discontinuance of treatment. A few hours of digital compression completed the cure. Such a result by such simple means (rendered painless through chloroform) is highly satisfactory and gratifying, whether it be considered from the standpoint of modern practice or from that of a hundred years ago, when the cure of aneurism was almost hopeless.

R. M., a healthy-looking man, aged twenty-six, formerly in the navy, was admitted on May 22nd, with an aneurism of the left popliteal artery, first noticed two months before. There was no history of injury, but a well-marked one of syphilis existed. The left knee measured in circumference an inch and a quarter larger round than the right, and there was, moreover, slight cedema, with considerable pain in the affected leg. The heart and the other arteries were, as far as could be ascertained, healthy.

May 26th. At 9.20 a.m. the affected limb was lightly covered with a flannel roller, and Esmarch's elastic bandage was applied firmly from the toes to about the head of the fibula. The patient then stood up, and the bandage was put on from about three inches above the patella up to the groin, at which spot the elastic ligature was applied, so as completely to stop all pulsation in the aneurism below. By these means an interval of seven inches opposite the aneurism at the knee was left unbandaged.—At 9.30 there was great pain in leg, and half a grain of morphia was given subcutaneously; ten minutes later the pain became intolerable, so that the patient was placed under the influence of chloroform, and kept so until the bandages were removed at 10.30 (eighty minutes after application), when the femoral artery was compressed in the groin. The sac now seemed to be more solid than before, but when the femoral artery was allowed to beat once, a distinct, although feeble, wave could be felt in the aneurism. Digital compression in the groin was, therefore, begun, and kept up energetically by Messrs. Rowe, Shawn, Spencer, and others.—At 4.30 p.m., after six hours of digital pressure, the aneurism was examined, and when the femoral artery was allowed to beat, the sac was found to be quite firm, and free from all pulsation. For the sake of safety, however, compression in the groin was continued until 10 p.m., when it was finally stopped.

The limb was wrapped in cotton wool, elevated, and a quarter of a grain of morphia was given. He had a good night's rest, and the next day the limb was warm and the sac hard; behind the latter a small anastomosing artery could be felt, and another a little to the inside of the patella. The heart-sounds were normal.

On June 8th the sac was both harder and smaller. The

patient got up from bed nine days later. He was able to walk with ease, complaining only of slight numbness in the leg. Neither the posterior nor the anterior tibial artery could be felt to beat. He left the hospital on the 18th.—*Lancet*, July 19, 1879, p. 81.

36.—THE BLOODLESS REMOVAL OF VASCULAR TUMOURS.
By N. DAVIES-COLLEY, Esq., M.C., Assistant-Surgeon to Guy's Hospital.

For many years I have been in the habit of resorting to excision in preference to the other modes of treating nævi; and I have found that in a large proportion of cases primary union follows, while in the instances in which this result is not attained recovery is, as a rule, more rapid, and the subsequent scar less visible, than when the ordinary methods of ligature or cautery, are adopted. The operator is, moreover, much more certain of removing the whole growth than if he employs the cautery, in which it is often very difficult to hit the mean between an incomplete operation and unnecessary mutilation. In the application of ligatures a great deal of pain is entailed upon the patient until the ulceration set up has freed the tissues from constriction. I have also had cases which show that this treatment is not always devoid of danger.

A healthy baby, æt. 9 months, was brought to me as an out-patient, with a subcutaneous nævus a short distance below the notch of the sternum. It was of an oval shape, $1\frac{1}{4}$ inches from above downwards, 1 inch from side to side, and raised $\frac{1}{4}$ inch. I strangulated it by means of two subcutaneous silk ligatures. A little suppuration followed, and the nævus was cured without any destruction of the healthy skin which covered it. But in a week, erysipelas came on together with great fever and prostration. For two months the attack lasted, and the child did not recover until the inflammation had visited in turn every portion of its surface.

The only drawback in excision has been the hemorrhage which is occasionally rather severe for a weakly infant, however rapidly and carefully the operation is performed. Last autumn I read in the *British Medical Journal* (vol. ii, 1878, p. 205) an interesting address upon surgery, by Mr. Wheelhouse of Leeds, in which he stated that Mr. Teale had removed a large nævus from the cheek of a child with a very small loss of blood, having first surrounded its base with a subcutaneous ligature, at the suggestion of his house-surgeon, Mr. Vernon. Soon afterwards a baby, 10 weeks old, was brought to me with a large nævus on the back of the neck. It was nearly circular with a diameter of an inch, and an elevation of about half an

inch. The greater part was subcutaneous. As it was important to remove it in as bloodless a manner as possible, I decided to employ a method which had occurred to me as an improvement upon Mr. Teale's temporary ligature. I first passed three strong hare-lip pins under the base of the tumour so as to cross one another about its centre, with their points of entrance and exit at least a quarter of an inch outside its circumference; I then took an ordinary india-rubber drainage tube, and wound it round underneath the heads and points of the pins, until it was evident that the circulation in the nævus was completely arrested. It was now quite easy to dissect back the healthy skin, by which part of the vascular tissue was covered, and to remove the affected skin together with the subcutaneous portion of the growth. There was no difficulty in distinguishing the limits of the disease, and not a drop of blood was lost during the excision. On removing the pins, a sharp hemorrhage ensued, but this was readily controlled by pressure. Silver sutures were used to draw the edges together, and boracic acid lint was applied. In three days the sutures were taken out, and primary union was found to have followed along nearly the whole line of the wound. The rest healed rapidly, and a small linear cicatrix was all that remained to indicate the site of the tumour.

Since then, I have frequently adopted the same method of operation with some slight modifications. When I have thought it desirable to avoid all hemorrhage, I have placed a pad upon the wound at the moment of withdrawing the pins, and then applied firm pressure. An open wound is left to granulate up, and there is no loss of blood, except, of a few drops which soak into the pad. With this treatment there is also a marked absence of pain, as the child is spared the tension and irritation produced by sutures.

The plan, however, which I now use most frequently, is the following. I first take two strong needles threaded with wire sutures and transfix the base of the tumour, the needles lying parallel to each other, and perpendicular to the long axis of the nævus, with their points of entrance and exit at least a quarter of an inch from its margin. A hare-lip pin is next introduced in the same way but at right angles to the needles. After winding the india-rubber tube as before, beneath the exposed extremities of the needles and pin, I dissect out the growth, leaving the skin over it, if it seems but little affected. The next step is to draw the needles out by means of a forceps, so as to leave the wire sutures in their place. Then after bringing together the edges of the wound by twisting the wires, the tube and pin are withdrawn, a pad of boracic acid lint applied, and gentle pressure exerted by strapping and a bandage. No blood

is lost during the operation, and after the dressing has been applied it is rare to find that so much hemorrhage occurs as to stain the surface of the pad, which consists of four or six thicknesses of lint. More bleeding than this I have never yet seen. The first dressing is usually four days after the operation. I then remove the wires, and reapply the boracic acid lint. In about half of my cases, primary union has followed, and the wound has been completely healed at the end of a week. In the rest, more or less of the wound has gaped, and repair by granulations has rapidly taken place. I am of opinion that the attempt to control the hemorrhage after the removal of ordinary nævi, by means of torsion or the application of ligatures to the vessels, often causes delay in the recovery on account of the bruising of the tissues by the forceps, or the irritation set up by the ligature. In the case of large nævoid or vascular growths, it would of course be unsafe to rely upon the pressure of a pad. I consider, however, that in these cases the method I have described of controlling the hemorrhage during the operation will be found of great service. The prevention of the hemorrhage after the operation may be effected by the application of ligatures or cautery before the drainage tube is unwound.—*Guy's Hospital Reports*, vol. xxiv., 1879, p. 437.

37.—REMARKS ON SILKWORM-GUT SUTURES.

By J. HOPKINS WALTERS, M.R.C.S.Eng., Reading.

The chief excellence of fishing-gut consists in its causing little or no irritation when embedded in the tissues. The way in which the latter tolerate its presence is wonderful, far surpassing either fine silk or silver wire, and, being perfectly soft while contained in moist structures, it remains pliable, admitting of, and participating in, the movements of these, instead of being stiff and resisting like wire. Another valuable quality is its comparative indestructibility, in this greatly differing from catgut, which, after a few hours, becomes completely softened and disintegrated, and finally incorporated with the surrounding tissue.

Fishing-gut maintains its integrity for many weeks, its strength seeming in no way impaired after removal. Its peculiar structure seems to render it almost as incapable as wire of becoming impregnated with the discharges from wounds which so often make silk injurious, if not absolutely dangerous. When I first used this material in 1873 I always steeped it for a minute or so in glycerine of carbolic acid, but have neglected to do so for a long time past, and do not find that it makes any difference.

My only excuse for mentioning the following trifling cases of

minor surgery is the length of time which elapsed, through the patient's neglect, between the introduction and removal of the sutures.

Case 1.—Scalp wound, five inches long, caused by blow with blunt instrument. Treated by continuous silkworm-gut suture covered with a piece of dry lint. Third day, wound healed throughout by first intention. No irritation round stitch-holes. Wishing to see whether silk-worm gut would be absorbed like catgut, I did not remove the suture, and told him to come again in a week. He did not come. I met him two months afterwards, when he told me that the stitches remained for *six weeks*, causing neither pain nor discharge. At the end of this time, finding their presence inconvenient when combing his hair, he got his sister to pick them out with a needle.

Case 2.—Phimosis, with sub-preputial chancre. September 10, 1877: Prepuce slit up, and mucous membrane and skin sewn together with continuous silkworm-gut suture. Patient went out partridge-shooting on the third day, and continued his sport regularly, walking long distances daily. I removed the stitches November 14, they having remained in this sensitive part for *two months* without causing pain or inconvenience in spite of much walking exercise. There was no sign of irritation; the gut was perfectly sound and unchanged, and not one drop of pus oozed out on its removal.

Case 3.—August 31, 1878: Fall from dog-cart on to heap of stones. Wound running whole length of eye-brow, injuring supra-orbital nerve, and small piece of frontal bone chipped off. Silkworm-gut continuous suture covered with strip of lint and styptic colloid. September 2: Found patient had gone shooting. He did not have the gut removed until the last week in October, having worn it without feeling it for *two months*. The gut was unaltered.

I am not aware of any other material that can be introduced into and remain embedded in the tissues for so long a time without causing suppuration or else losing its integrity. I therefore believe that when these properties of silk-worm gut are more widely known it will be highly valued by surgeons. Mr. Bryant writes me that he has used nothing else for plastic operations for the last ten or twelve years. For these it seems peculiarly suitable, as also for all wounds involving or operations within the peritoneal cavity, for stitching intestine as in the operation for artificial anus, and, above all, for ruptured perineum. I imagine also that no better material will be found for tying the pedicle of an ovarian tumour, but of this I have had no experience. The gut is very strong, and can be obtained of varying thickness, from the finest hair to stout salmon-gut; and this, if used as a ligature, might be doubled or twisted as

is done in some fishing casts. The thin gut can be used without any preparation; but the stouter kinds ought to be kept in water a few minutes to make them soft and more easy of manipulation. It quickly dries and resumes its stiffness.—*Medical Times and Gazette.*, June 28, 1879, p 701.

38.—ANTISEPTIC TRANSFUSION OF HUMAN BLOOD IN
A PATIENT THE SUBJECT OF SECONDARY
HEMORRHAGE; CURE.

By Dr. WILLIAM MACEWEN, Surgeon and Lecturer on
Clinical Surgery, Royal Infirmary, Glasgow.

The following case of transfusion of human blood is worthy of record, on account of the antiseptic precautions which were adopted and the complete success of the operation.

The patient was a man, twenty-three years of age, on whom lithotomy was performed for the removal of a large spiked oxalate-of-lime calculus. There was little bleeding as an immediate sequent of the operation, and it was completely arrested before he left the table. Half an hour after having been put to bed, the house-surgeon found him to be very comfortable and in a good general state; there was then only slight staining on the sheet under the pelvis. Two hours and a half after he was found to be in a state of complete depletion, from a profuse secondary hemorrhage which had ensued. The wound was at once plugged. Notwithstanding every attempt to resuscitate him by stimulating drinks, enemata, &c., it was evident, at the end of three-quarters of an hour, that ground was being fast lost, and it was clear to all present that, if his life was to be saved, something more radical was necessary. Transfusion of blood was proposed. A patient who suffered from injury to the right great toe, and who otherwise was strong and healthy, after being apprised of the slight risk he ran in giving a portion of his blood, freely offered it to his fellow.

The lithotomy patient was then in the following state. He was semi-insensible, could not speak, pulse at the wrist imperceptible, surface of the body blanched and bedewed with a cold perspiration, the lips cream-coloured, and the conjunctival vessels no longer visible. Occasionally he gave a restless feeble toss, accompanied by a deep inspiration.

An attempt to find one of the large veins on the right arm being unsuccessful, the median cephalic of the left was chosen, and half an inch of its length exposed. An assistant was desired to place the finger of one hand to the distal side of the exposed vein, and to maintain pressure on that part throughout the operation, so as to prevent loss of blood; and with a finger of the other hand, placed on the proximal side about half an

inch above the part selected for opening the vessel, he was to occlude the vein when required. The arm was held well up, so as to empty it of any blood which it might contain. It was also maintained considerably above the patient's body, for three reasons: first, to facilitate the flow of the transfused blood into the trunk; secondly, to prevent the entrance of air into the body, as the syringe would, with the arm in this position, be necessarily held perpendicularly, with the nozzle downwards, and all contained air would remain at the top of the instrument; and, thirdly, to enable any air to escape which might be in the space intervening between the opening in the vein, and the occluding finger of the assistant, on the proximal side. The vein was then opened. Then phlebotomy was performed on the healthy man, the blood being received into a small warm carbolised vessel from which it was at once drawn into a warm carbolised three-ounce syringe, having a narrow nozzle. When full it was inverted and the piston pressed, so as to expel any air, and the nozzle was then introduced into the vein. A quantity of blood was first injected into the space in the vein, between the occluding finger on the proximal side and the opening in the vein itself. When this was done the pressure on the proximal side was removed, and the contents of the syringe were slowly injected, until only a couple of drachms remained. The pressure of the assistant's finger was again applied, and the syringe removed. It was then washed in 1 to 80 carbolised watery solution, recharged, and the blood introduced as before. The tin into which the blood flowed was kept free from clot, and several times a fresh cup was substituted. The arm from which the blood was drawn, as well as that into which it was injected, were kept constantly under the spray, and the blood itself, from the time it left the one arm until it was injected into the other, was either exposed to the carbolised spray or in contact with carbolised instruments; so that the whole transfusion was thoroughly antiseptic.

With the exception of the transfusion being performed antiseptically, the other details of the operation were nearly the same as those adopted by Mr. Lister in a case in which he performed transfusion, while I acted as one of the house-surgeons in the Royal Infirmary. That case was reported by me in the Glasgow Medical Journal for Nov. 1869.

Just before the transfusion was begun several of the house-surgeons hinted that the patient "had slipped away." His heart, however, was heard to respond, and the blood was injected. Shortly after the transfusion the gentleman who had his finger over the radial said that, from being imperceptible, it had returned gradually, and had increased until it was distinctly felt. Half an hour after the face had resumed a slight

redness, and heat began to be restored to the surface of the body. There were no rigors after the transfusion. Without entering into further detail, it may be said that he slowly but perfectly recovered, and is now a strong healthy man. He was shown at the Pathological and Clinical Society nine months after the operation, and is still quite well and at work.—*Lancet*, July 5, 1879, p. 4.

 ALIMENTARY CANAL.

39.—ON A PAINLESS METHOD OF EXCISING THE WHOLE TONGUE.

By RICHARD BARWELL, Esq., F.R.C.S., Surgeon to and Lecturer on Surgery, at Charing-Cross Hospital.

The disease (in the case in which this operation was performed) was a large epithelioma situated as far back in the organ as the anterior pillar of the fauces, occupying chiefly the left side; that is to say, the tumour itself and the ulceration were confined to that side, yet the condition called ichthyosis extended across and some distance on the right of the raphe. Now my late colleague, Mr. Fairlie Clarke, pointed out some years ago that this morbid state is the immediate precursor, or, indeed, the first stage of epithelioma. To take away a part of the tongue and to leave behind an ichthyotic portion would be a grave mistake. It was necessary, therefore, in this case to remove the whole breadth of the organ from a point very near the epiglottis. I desire to fix your attention upon the method I adopted, upon its ease both to surgeon and patient, and upon the absence of bleeding or external mutilation; especially as you will find in works on surgery, much used by students and practitioners, certain methods of operation described and figured which I entirely disapprove. For instance, Regnoli's operation consists in cutting away the whole floor of the mouth by incisions along the middle line and round the body of the lower jaw, then dragging the tongue through the opening down upon the front of the neck, and severing it from its base. Another method is to split soft parts and jaw from the mouth to the hyoid bone, and by dragging the parts asunder to lay bare the root of the tongue. These operations cause much hemorrhage, are very dangerous, and produce horrible mutilation. I have no hesitation in saying that they should only be mentioned as I mention them now—namely, as relics of a past and, in this particular region, of a barbarous stage of surgery. Even the division of muscles &c. passing between the jaw, hyoid bone, and tongue, as suggested by Sir James Paget, so as to enable the surgeon to drag the last-named part out of the mouth, is quite unnecessary,

because, as you have seen, the tongue can be removed *e situ* with the greatest ease as far back, if necessary, as the epiglottis; nay, if it were desirable, that valve could, as far as the mere mechanism of the operation is concerned, be removed with the tongue from the hyoid bone.

The method itself is very simple. The instruments required are a small scalpel, one or two Liston's needles, and an *écraseur*, or better, two *écraseurs*. When the patient is well under the influence of the anæsthetic, place a gag between the jaws, draw the tongue a little forward, and pass through the raphe a string, with which the organ is to be simply controlled, not dragged out of the mouth, which must be avoided. An incision, about a quarter or a third of an inch long, is now made from the hyoid bone forward, and strictly in the middle line. Thus far you will see my operation resembles Nunneley's, except that my incision is further back and shorter; but from this point the methods differ, for that surgeon passed by means of a seton-needle the loop of an *écraseur* chain into the floor of the mouth through the frenum of the tongue, and then dragged the part to be removed forward through the loop; and, although he could remove considerable parts by these means, he could hardly get at the whole organ, and I think his opening into the mouth too short and direct, nor did he eliminate pain.

By my method, when the raphe of the mylo-hyoid has been divided, the knife is laid aside, the genio-hyoid and genio-hyoglossus muscles are separated from their fellows by the handle of the scalpel or by the finger if the surgeon have a small fingertip, and the root of the tongue is readily reached; but the mouth is not to be opened here. An armed Liston's needle is now placed in the wound, and the forefinger of the other hand between the diseased side of the tongue and the jaw, as far back as it will go—viz., a little beyond the last molar tooth,—and to this point the needle is guided, taking care to keep it rather nearer to the bone than to the side of the tongue; here it pierces the mucous membrane, enters the mouth, and the thread, being released, is withdrawn, a loop of cord being left behind. The same thing is then done for the other side, except that here a loop in the mouth is unnecessary. The *écraseur* is now taken in hand; it must have one end of the wire detached and bent into a sort of hook at as sharp an angle as the material will bear. Tie an end of the last placed thread in the bend of this hook; then by traction on the other end, that in the mouth, draw the wire along the track of the needle. When the metal appears in the mouth just beyond the last molar tooth, pull the wire gently through till the nozzle of the *écraseur* is close to the supra-hyoid wound; then detach the thread and pass the wire hook into the loop of twine that enters the mouth on the

diseased side of the tongue, and by gentle traction draw the metal from thus far back in the mouth, out at the hyoid wound, and attach it to the body of the instrument. Before screwing the wire tight, pass a finger along the dorsum of the tongue and ascertain its exact position. I am not afraid of its lying too far forward—it might easily, without care, sit too far back, also it might slip away from the desired place as the screw is used; therefore, having fixed the exact line along which the tongue is to be severed, I place my finger where that line intersects the raphe on the dorsum of the tongue; to it I pass the Liston's needle, letting its point project a line or two, and taking care that the wire lies behind it; by this means the écraseur can be guided exactly along the required plane. When the base of the tongue has been cut through, and the wire has come out at the wound, the loop of the same or of another écraseur is passed over the tip of the tongue into the line of incision, and the tissues, small in quantity but very vascular, which attach the tongue to the floor of the mouth, slowly cut through, when the whole organ is severed, and is removed from between the lips.

Now to recall your attention to the man himself. He lost during the operation not more than ten drops of blood, and none since. He has in front of the hyoid bone a very small scar of an already healed wound, and no other external mutilation. He has lost the whole of the tongue, well clear of the disease, as you see by the specimen, and within a line or two of the epiglottis; yet he has no fever, his temperature is normal, and he takes tepid liquids without difficulty. Whenever I have asked him if he is in or has suffered any pain, he invariably answered in the negative. It seems strange, at first sight, that an organ so sensitive as the tongue can be removed without the production of a moment's pain, especially as a good deal of suffering follows the usual modes of excision; yet, when we have considered the matter together, you will see that this is a necessary result of my method of operation. By avoiding any dragging of the tongue forward, but, on the contrary, getting the écraseur wire round it *in situ*, and by keeping that wire, just previous to its entrance into the mouth, rather near though not close to the ramus of the jaw, I divide the sensory nerve of the tongue—the lingual-gustatory—close to the bone; it then retracts into its groove, and the whole wound must of necessity be insensible to pain. Therefore the man could immediately after the operation take abundance of liquid nourishment, avoided fever, and the part has rapidly healed. I would suggest, though I have not yet had an opportunity of reducing the proposal to practice, that when a less portion of the tongue has to be removed the lingual-gustatory nerve of one or both sides, according to the extent of amputation, might with advantage

be divided on the ramus of the jaw.—*Lancet*, April 19, 1879, p. 549.

40.—ON THE PAINLESS AND BLOODLESS METHOD OF
EXCISING THE WHOLE TONGUE.

By Dr. F. A. PURCELL, Assistant-Surgeon to the Cancer
Hospital, Brompton.

I quite agree with Mr. Barwell, and recognise the full force of what Mr. Fairlie Clarke pointed out some years ago, that if epithelioma of the tongue, ulcerated or not, exist on one side only, yet a condition called ichthyosis almost always—that is, if the tumour is of any size—extends across and to some distance into the opposite side; that this morbid state is the immediate precursor, indeed, the first stage, of epithelioma; hence the reason that to take away a part of the tongue and leave behind an ichthyotic portion, would be a grave mistake. I am sure it will be conceded that removal of the tongue, wholly or in part, for cancer, is a justifiable and a wise surgical proceeding, and that the sooner the operation is performed after the diagnosis of the disease has been made, the greater are the prospects of a long immunity from the disease, or even of a complete cure; for there are few if any local cancers that are the cause of more local distress if left alone, and there are few operations for cancer that give greater comfort than those on the tongue; therefore it behoves us to seek for the least painful, most manageable, and most effective plan, as also the least bloodless.

I must take exception to Mr. Barwell's remark, however, when he comes to call his operation "a painless method." The lingual-gustatory nerves get divided when his patient is under the influence of the anæsthetic, and therefore insensible to pain, and get divided in the same place by whatever method is employed when the whole tongue has to be removed. But Mr. Barwell throws out a suggestion that when a less portion of the tongue has to be removed the lingual-gustatory nerve of one or both sides might with advantage be divided on the ramus of the jaw.

The division of the sensory nerves of the tongue was first proposed by Hilton, who practised section of the gustatory branch of the fifth nerve with a view of relieving the pain of a cancerous ulcer, and enabling the surgeon to apply ligatures for the removal of the cancer to a part that has been deprived of all sensibility. Moore also adopted it, and it undoubtedly accomplishes the object for which Hilton originally proposed and practised it, more especially so far as the relief of pain and diminution of salivation are concerned, and it deserves to be

considered one of the most efficient modes of relief to the suffering produced by cancer of the tongue in all cases in which an operation for the removal of the disease is not desirable or practicable. Moore's method is to be preferred to that of Hilton (for the former divided the nerve further back), and becomes more applicable when cancerous disease lays hold of the floor of the mouth. The good effect of the operation is instantaneous. I have found that pain ceases in the tongue, ear, face, and head, and that the flow of saliva is greatly diminished; the relief besides, is continuous, the nerve appearing not to reunite.

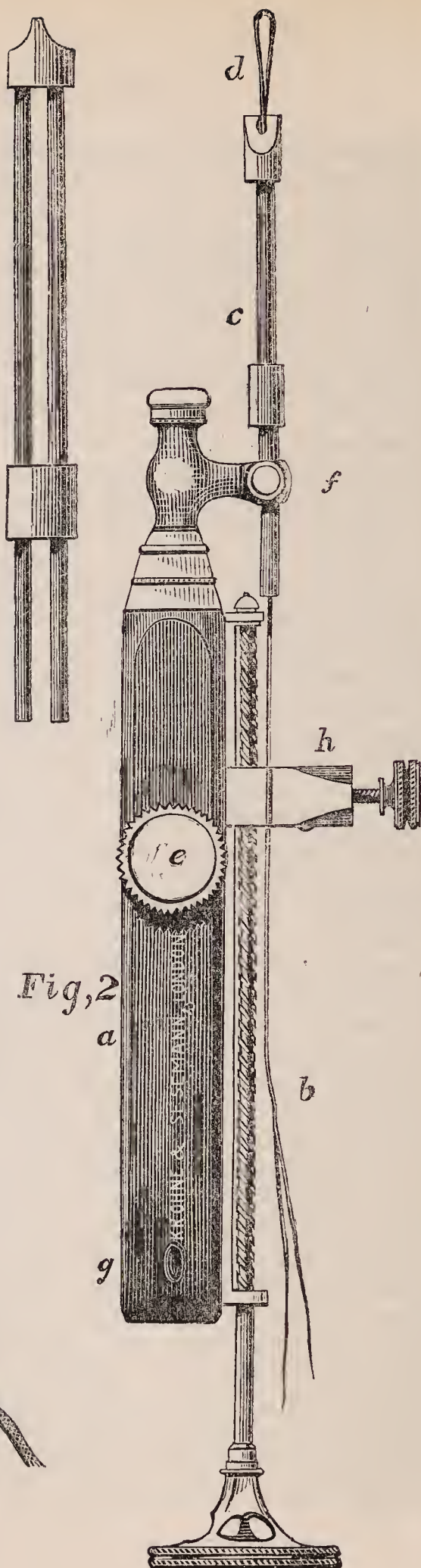
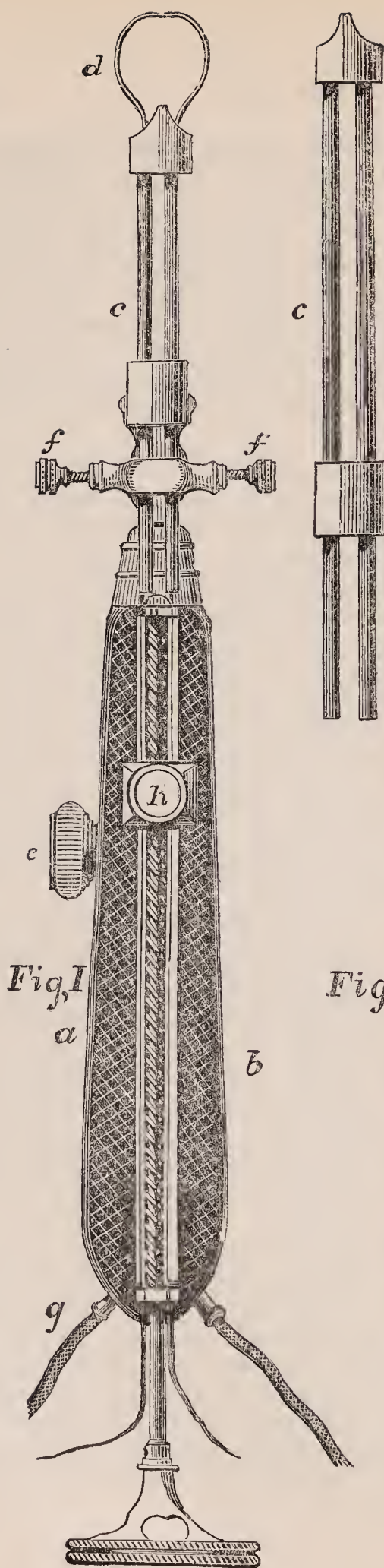
To Chassaignac, however, are we indebted for inventing and successfully using the wire *écraseur*, and by its means first removing the tongue from the interior of the mouth without any external incision; this was a great advance in surgery.

Cloquet, Arnold, and others strangled the tongue by making an incision in the hyoid region, carrying the ligatures by means of long needles through the base of the tongue, and then drawing them tight through the aperture in the neck so as to constrict the diseased mass, and cause the organ to slough away.

Regnoli, of Pisa, published in 1838 a description of a method by which the whole tongue could be successfully removed. He made an aperture into the floor of the mouth in the mylo-hyoid space; the tip of the tongue being seized, the organ was drawn to its full extent down on the anterior part of the neck, when the whole tongue was shaved off from the base of the epiglottis and hyoid bone by means of the *écraseur*.

Nunneley, of Leeds, devised an ingenious mode of applying the *écraseur*, so as to remove large portions of the tongue as far back as the hyoid bone. I will give a short description of his operation:—It consists in passing the chain of the *écraseur* through the centre of the mylo-hyoid space by a needle into the mouth close to the *frænum*; two or three curved and strong hare-lip pins are now passed deeply into the tongue, obliquely behind the seat of the disease, their points being made to project forwards below the organ, so as to prevent the chain from slipping; the loop is now gradually tightened, and the tongue cut off obliquely from behind forwards.

I must not go too far into the literature of this subject, for if I do I may get into trouble, and fail to give credit where credit is due. I beg to refer to Mr. Thomas Bryant's interesting lecture given by him at Guy's Hospital, and reported in the *Lancet* of February 28th, 1874, under the head of "Bloodless Operations, as illustrated by the use of the Galvanic Cautery." In giving a description of the instruments, and in showing a woodcut of some, I shall have to apologise to Mr. Bryant for following him to a great extent, my only excuse being that the



subject is well worthy of repetition. Mr. Bryant there states that Dr. Middeldorpf, of Breslau, are we really indebted for the introduction of the splendid battery and set of instruments which have rendered the galvanic cautery an agent of practical use; for in 1854 he published a monograph on the subject, and in 1868 Dr. Middeldorpf sent over to Dr. Hermann Beigel a battery and a set of instruments, and through Dr. Beigel's courtesy Mr. Bryant had an opportunity of using them. Since 1875 we at the Cancer Hospital have used the galvanic *écraseur*.

I will now explain the instruments, and pass through the different steps of the operation for removal of the whole tongue, as performed by our senior surgeon (Dr. Marsden) in 1875, with such success, that with one or two modifications it is the plan still adopted by him, myself, and my colleagues. To commence, then, with the necessary appliances. The battery in use is one of Grove's with five cells. Messrs. Krohne and Sesemann recommend Bunsen's (carbon and zinc) of four cells. The battery must be in good working order, or the required heat will not be maintained. I may here say that if the instruments are imperfect some break in the galvanic current is sure to take place. Before operating, the surgeon should therefore test the battery and instruments that are to be employed, and in this way save trouble and prevent disappointment or possibly failure.

Fig. 1 gives the front view of the galvanic *écraseur*, and Fig 2 its side view.

The instrument consists of four separate and separable parts. *a*, The handle; *b*, a screw on which travels a clamp (*h*); *c*, canulæ through which the wire passes; *d*, platinum wire loop; *e*, ivory "interrupter" to connect or break the galvanic current by slight pressure of the thumb; *ff*, sockets into which the canulæ can be fixed by screws; *g*, receptacle for connecting wires from battery; on the long screw (*b*) rides the clamp (*h*), between the jaws of which the ends of the platinum wires can be fixed by a screw. A gag, artery forceps, ligatures, a strong whipcord ligature, scalpel, and a couple of Sims' wire guides (Fig. 3) complete the apparatus.

The patient being placed on the table, and brought under the influence of the anæsthetic, a vertical submental incision (after Nunneley) is made in the centre of the mylo-hyoid space, and the opening freed into the floor of the mouth in front of the frænum of the tongue. Slight bleeding takes place. The passage is enlarged by means of the two forefingers working against one another sufficiently to allow the nozzle of the canulæ (*c*) to pass through. The gag is then placed between the jaws, and the mouth kept open; the whipcord ligature




FIG. 3. is passed through the raphe about an inch from the tip of the tongue. The canulæ (c), freed from the handle, and with the wire loop (d) drawn home, the nozzle of which is then passed through the submental incision up into the mouth. The wire-loop is then, by means a forceps, drawn out from the nozzle of the canulæ to form the loop (d), through which the tongue, with its whipcord ligature, is introduced, and the tongue being gently drawn forwards by an assistant the operator carries the loop (d) back over the root of the tongue close up to the epiglottis. The assistant now gently pulls on the free ends of the platinum-wire, so as to tighten the loop (d), which the operator gets into position by means of his two forefingers, and which he retains *in situ* by means of the wire-guides (Fig. 3), one on either side of the tongue. Now the canulæ and the free ends of the platinum-wire are passed through the sockets (ff) of the handle (a); the screws (at ff) are then screwed tight, and the canulæ fixed. The ends of the wire are then fixed between the jaws of the clamp (h) by a screw. The screw (b) on which the clamp (h) travels is then gently worked sufficient to fix the loop round the root of the tongue. All being satisfactory, the connecting-wires from the battery are placed and fixed in the receptacles (g), the ivory-nut (e) is now screwed home, the galvanic current is completed, the loop reddens, and the *écraseur* is at work. The screw (b) is then gently worked, and above all, the process of tightening should be very slowly performed. The wire of the *écraseur* being screwed home only as it becomes loose by cutting through the tissues, any force may break it. The tongue is gradually burnt through in from seven to ten minutes, and, being detached, is drawn out of the mouth through the lips by the assistant. The operator immediately breaks the current by unscrewing the ivory nut (e), and removes the nozzle of the canulæ from the mylo-hyoid space. If the lingual arteries begin to bleed they are easily seized and tied. The tongue is found severed obliquely from behind forwards. The gag is then released, and the submental incision brought together with strapping; the operation is complete, and the patient taken to bed.

Whenever bleeding follows the operation that has been described, it is from one of two causes: the wire cautery has been used at too great a temperature, or has been screwed up too rapidly. Hemorrhage is, however, easily restrained, either by the application of iron-lint or ice.

The introduction of the galvanic cautery has so benefited and simplified the operations on the tongue that the surgeon can by

its means isolate the disease with ease. He can satisfy himself that the whole tumour is under his control; his operation is bloodless, and he need in no way expedite his movements. Before the introduction of the galvanic *écraseur*, operations on the tongue were very rarely performed. Now, however, it is different; they are of common occurrence, and by these means operations on the tongue for cancer, I am inclined to believe, are more successful than when they are undertaken by any other means.

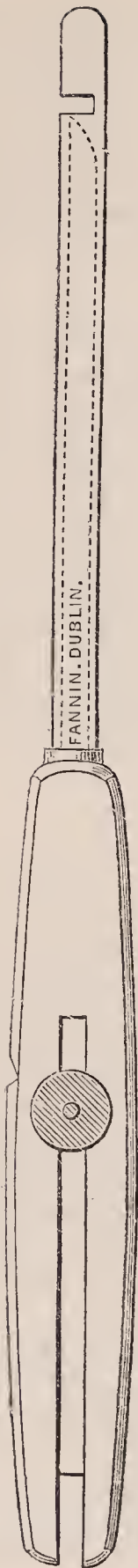
Mr. Barwell, in his plan, uses the wire *écraseur*, and in a complicated manner, to my mind, gets one end of the wire passed into the mouth through a submental incision in the mylo-hyoid space by means of a previously-passed piece of cord, and, encircling the tongue, gets it withdrawn through the hyoid space at the opposite side of the tongue by means of a previously-passed cord loop, endeavouring to include the lingual-gustatory nerves in the wire. All being secured, it crushes through the tissues of the tongue, severing the sensory nerves "close to the bone." No doubt the wire *écraseur*, applied as Nunneley of Leeds performed his operation, comes next to the galvanic cautery, but the galvanic cautery has this great advantage over all others—that it gently burns through the tissues, without any dragging or tearing of the neighbouring structures.—*Lancet*, July 5, 1879, p. 2.

41.—A FORM OF HERNIA-KNIFE.

By P. J. HAYES, Esq., F.R.C.S.Ed., Surgeon to the Mater Misericordiæ Hospital, Dublin.

The little instrument which I have designed, under the impression that it will be found an useful addition to the armamentarium of the operating surgeon, possesses no claim for position in the class of surgery-made-easy contrivances; it is merely a form of hernia-knife, with the aid of which, I anticipate, the surgeon can safely and efficiently overcome strangulating pressure in cases of femoral hernia where necessity for incision through the base of Gimbernat's ligament exists. This may be thoroughly accomplished, even in the event of an anomalous obturator artery taking a course directly across the line of section.

The essential characters of the knife are few and simple. The outer part of the instrument consists of a long slender sheath of steel. This sheath is compressed so as to present two flat sides or surfaces and two thin rounded margins. The distal end or extremity is also smooth and rounded. At a quarter of an inch from the extremity, one margin is interrupted by a transverse notch or fissure nearly one-eighth of an



inch in depth, and rather more than the sixteenth of an inch wide. Within the sheath, a thin flat blade having blunt edges, but with an oblique, sharp, chisel end, is arranged to slip up and down after the manner of a pocket slide-pencil. The instrument is to be employed in the following way. The blade having been drawn down, the sheath is applied to the finger on the flat, like an ordinary hernia-knife, and guided through the inner part of the crural ring. When sufficiently within the aperture, the instrument is to be turned with the notched margin inwards, so that the aperture shall correspond to and receive the base of Gimbernat's ligament. I need hardly say how, by a little upward and downward movement of the sheath, the surgeon can cause the edge of the ligament to enter the notch, and so be presented for incision when the chisel-blade is pushed up. Of course, one small cut in the base of Gimbernat's ligament may not prove sufficient to permit reduction of the hernia, but the wound can be enlarged to the required extent by withdrawing the blade a little and pressing inwards the sheath until a fresh portion of membrane becomes stretched across the opening and exposed to the blade during another upward movement. It is clear that any vessel resting upon the upper surface of Gimbernat's ligament must be pushed inwards by the edge of the sheath immediately above the notch, and so protected from the possibility of being wounded. Also, the operator can regulate to a nicety the extent of incision through the ligament, and preserve his patient from all risk of being left with an unduly enlarged femoral ring.

The accompanying woodcut represents the instrument as made by Messrs. Fannin and Co., of Grafton Street, Dublin. The dotted line shows the position of the blade within the sheath when ready for use.—*British Med. Journal*, June 21, 1879, p. 931.

42.—ON THE NATURE, SYMPTOMS, AND TREATMENT
OF CONSTRICTED OR STRANGULATED HERNIÆ,
REDUCED “EN BLOC.”

By Professor SPENCE, Edinburgh.

After a hernia has been reduced and symptoms of obstruction continue, doubts may arise as to whether these symptoms may not depend on some internal strangulation or volvulus coincident with the presence of a reducible hernia; and in such cases mesial incision would be the best plan of operating. But if I can show, from a consideration of the relations and state of parts, and from what I have observed of the symptoms and conditions of herniæ reduced *en bloc* on which I have operated, that the diagnosis is not so difficult as seems to be supposed, then the indication for the performance of the old or direct method in preference to mesial incision in herniæ reduced *en bloc* will become evident.

As regards the state and relation of parts in a hernia reduced *en bloc*. If we consider the way in which the sac is formed by the parietal peritoneum, it is obvious, first, that when a portion of gut constricted by the neck of the sac, or by the condensed tissue around the neck of the sac, is pushed back from the canal through which it had protruded, the mass must lie between the parietal peritoneum and the abdominal parietes in close proximity to the upper part of that canal. In some cases the fundus of the sac may not be pushed quite out of the canal. In those cases, however, in which no tumour can be felt, the constricted hernia has been pushed fairly through the upper opening of the canal, and lies between the fascia transversalis and peritoneum, with the body of the sac pressed aside, more or less bent upon its neck, and no longer corresponding to the axis of the deep ring or course of the canal, through which it formerly protruded. In the second place, it is impossible that the constricted mass can ever be pushed within the peritoneal cavity, because that could only be effected by invaginating the sac, and in doing that its contents must be everted and so relieved from constriction.

It is not difficult to understand how small herniæ, such as femoral, may be pushed up through the short canal within the abdominal parietes. The cases likely to give rise to doubt are cases of large inguino-scrotal hernia, in which the rupture has been reduced with some difficulty, perhaps, but with distinct sense of “gurgling.”

It may at first seem difficult to conceive of such a large hernial protrusion being pushed back constricted. The condition of the hernia in these cases requires explanation, as it is one not much alluded to in surgical works. It is one to which I drew attention in reference to a case of strangulated scrotal hernia on

which I operated in 1840. The peculiarity consists of the division of the sac into two cavities, a large upper part and a small lower compartment, with a narrow thickened orifice or canal of communication between them. In such cases the strangulation is at the constricted neck of what I may call the lower and smaller sac. Hence, whilst the contents of the larger sac are reduced with the sense of gurgling, it acts as a dilator, the small constricted mass follows and is pushed up within the parietes, still constricted.

Keeping in mind the relations and state of parts in herniæ reduced *en bloc*, I proceed now to connect them with certain symptoms which I consider characteristic of that accident which distinguish it from other causes of obstruction, such as internal strangulation and volvulus coincident with a reducible hernia.

In all cases of hernia reduced *en bloc*, we have the history of the presence of a hernial protrusion in the first instance accompanied by incipient symptoms of constriction. The apparent reduction of the protrusion with more or less difficulty, the continuance and gradual aggravation of the symptoms of obstruction, occasionally, perhaps, some slight remission of more urgent symptoms, but never any real relief. If the protrusion has been fairly pushed up within the parietes, then no tumour can be felt. Even in cases of large scrotal herniæ so reduced *en masse*, in which the dilated and now empty canal permits the finger or fingers to be easily passed up throughout its whole extent, nothing can be felt beyond a dull impulse on coughing.

There may be a greater degree of dulness on percussion and tenderness over the part of the abdomen corresponding to the deep ring, and above it, than elsewhere, if we have an opportunity of examining the patient soon after the mass has been reduced. But most usually when the surgeon is consulted the general tenderness and tension of the abdomen render the symptoms indistinct.

The condition or symptom on which, from experience and from consideration of the state of parts, I rely as diagnostic, is, that when the constricted protrusion has been pushed up from the canal completely within the parietes, no effort of coughing or action of the abdominal muscles can make the hernial swelling reappear. This I consider to be quite characteristic of the true nature of the case as distinguished either from a constricted hernia partially reduced from the canal, or from an internal strangulation coincident with a reducible hernia.

In the former case the patient may, by forcible effort of coughing, or by being made to sneeze, cause the hernia to descend, and then it comes down with a "bolt" *en masse*, and leaves little doubt as to the mode of treatment. In such a case, the fundus of the hernial tumour has still occupied the upper

opening of the canal, and its body still corresponded to the axis of the canal, and so there is nothing to prevent a certain degree or kind of force causing it to descend as I have described. In the case of a protrusion reduced *en bloc* within the parietes, it no longer corresponds to the opening or course of the canal. It is pressed aside, with the body flexed on the neck of the sac, out of the line of the canal from which it has been pushed, and therefore cannot be made to reappear.

In the case where the obstruction depends on the existence of an internal strangulation coincident with a reducible hernia, when the hernia is reduced there will be no difficulty in causing it to reappear, rather, on the contrary, there will be difficulty in retaining the hernial protrusion in consequence of the abdominal tension causing the free intestine to escape. This is what we see in cases of double hernia, when one of them becomes strangulated. Both herniæ are tense, the unconstricted one is reduced with some difficulty, and when reduced has a constant tendency to protrude, until the abdominal tension is removed by relieving the constriction on the strangulated hernia. I think it will be admitted that the conditions leading to the symptoms in both cases are, if not identical, strictly analogous. From what I have stated regarding the condition of parts, and from what I have observed in cases in which I have been consulted, and on which I have operated, I feel satisfied that there should be little difficulty in arriving at a decided diagnosis between an internal strangulation and a hernia reduced *en bloc*.

As to treatment, the sooner operative measures are resorted to the better, and as to the method of operating, I feel satisfied that the old or direct method is better suited for cases of hernia reduced *en bloc* than the mesial incision. Let us briefly consider the advantages and disadvantages of each method. In the direct method, by incising in the line of, and laying freely open, the canal through which the hernia formerly protruded, we are certain of finding the constricted hernia as it is fixed by its connexion with the peritoneum, and if the incisions be properly planned, we generally find it easily, and can readily bring it down into the canal. Then we deal with it as in an ordinary hernial operation, opening the sac and examining its contents, to satisfy ourselves of their condition, and of the propriety of reducing them or otherwise. Next, by dividing the constriction to a moderate extent, we feel whether the gut is adherent, and if not, we can draw it gently down so as to see the part which has been more immediately under the constriction (for that is the point of danger), and judge of its condition and its fitness for reduction. And all this is done whilst the parts are, as it were, outside the abdominal cavity,

and therefore with little or no risk of fæcal extravasation.

Again, if, as is too generally the case in hernia reduced *en bloc*, we find the intestine gangrenous or parts of it in a doubtful state, we can deal with it much more satisfactorily than if we had opened the sac from within the abdomen, and had to deal with gangrenous gut in that cavity. It not unfrequently happens in hernial cases in which constriction has continued for some time, that changes have begun in the gut which was directly under constriction, sometimes pretty firm through recent adhesions, with softening of the gut in the immediate vicinity. In other cases the state of the strangulated intestine may be such as to render its reduction of doubtful propriety, even if not adherent. We know that though the peritoneal aspect of the intestine is not gangrenous, that the mucous membrane at the seat of constriction suffers at an earlier period, and is liable to ulcerate and lead to perforation, with its fatal consequences, if the doubtful portion of bowel has been returned within the abdominal cavity. In such cases the proper plan is to relieve constriction freely, but not to reduce the doubtful portion of bowel immediately. Here it is evident that we can examine and deal with the contents of the sac much more safely and satisfactorily, by what I call the direct method than by a mesial incision, by which we can only reach the strangulated intestine indirectly, and cannot see or judge of its state at or below the constriction until we have freed and brought it into the cavity of the abdomen.

The mesial incision in cases of strangulated hernia reduced *en bloc*, besides being more dangerous than the old or direct method, seems to me to present no advantage whatever, except to resolve a doubtful diagnosis, which, for the reasons I have adduced, I do not think should exist. Mesial incision or gastrotomy in cases of internal obstruction is certainly proper and much more frequently performed than formerly, but even in such cases its results are not specially brilliant, whilst all who have performed it know that it is often troublesome, complicated, and dangerous.

It is true that in many cases, by making a limited incision, and taking care never to allow any large portion of distended intestine to protrude, but examining and returning it bit by bit until we arrive at the point of obstruction, we may accomplish our object without much exposure or manipulation of the intestine. In other cases, however, as in some cases of volvulus where the mass of intestines prevents us seeing and dealing with the obstructed portion, and in which the distended small intestines require to be for a time withdrawn from the abdominal cavity, or in cases where, in spite of our efforts to control them, the coils of distended intestines escape, we have in the mesial

incision formidable sources of embarrassment to the surgeon and danger to the patient, from which the direct operation for herniæ reduced *en bloc* is free.

But suppose that in operating by the mesial incision on a case of hernia reduced *en bloc* the surgeon is readily guided by the distended intestine to the seat of strangulation. What then? He has reached it indirectly; he cannot see or examine it even imperfectly without exercising traction to bring it from the inguinal or inguino-femoral region towards the mesial line, and here begins one of its greatest risks. It is said that extrication of the constricted hernial protrusion can be more easily effected by traction from within than by pressure from without, as in taxis. That is true under certain conditions, as when the protrusion is recent and not very tightly constricted, as in the earlier stage of strangulation. We know the value of exciting peristaltic movement of the intestine by enemata as an auxiliary to or to prepare for the taxis. It is on the same principle also that the old Indian method of reducing a hernia by means of a sheet or cloth passed across the abdomen and drawn from below upwards, whilst the patient is placed on an incline with the head down and the lower part of the body raised is successful in aiding ordinary taxis. But in cases of herniæ reduced *en bloc* the conditions are different. The supposition that the rupture has been fairly reduced too generally leads to temporizing treatment and loss of time before the danger is realized and the propriety of operative interference considered, and in the meantime the reduction *en masse* has intensified the constriction and aggravated the tendency to morbid alteration in structure. We have only to look at preparations from fatal cases of strangulated hernia to see what might often occur if traction were made from within to extricate the constricted intestine. In many cases the partially adherent and altered gut would give way at or above the seat of stricture, and its contents would be extravasated into the peritoneal cavity. Or if, as must frequently happen, the constriction was so tight as to require division by the knife from within, such a procedure would be attended with less safety and certainty as to the relation of the bloodvessels, but especially with greater risk of wounding the distended intestine below the constriction, as the surgeon could not push aside or guard it, as he does when he opens the sac and divides the stricture in the usual way from below upwards from without.

Then, in dealing with doubtful or partially gangrenous intestine, to which I have already alluded, we would be placed at great disadvantage in managing the intestine, whilst, if the state of the gut led to the formation of an artificial anus, the central abdominal region would certainly not be preferable to the groin.

Looking at the question from every point of view, I feel satisfied that the ordinary direct method of operating in cases of strangulated hernia reduced *en bloc* is the proper procedure, founded on true principles, and decidedly preferable to mesial abdominal section.—*Edinburgh Med. Journal*, Aug., 1879, p. 977.

43.—GLYCERINE IN THE TREATMENT OF HEMORRHOIDS.

By Dr. DAVID YOUNG, Florence.

During the past eighteen months I have used glycerine frequently in hemorrhoidal cases. I would suggest the following classification as being both strictly pathological and affording a simple clinical guide as to treatment, viz.:—1, External hemorrhoids, and 2, Internal hemorrhoids.

1. *External Hemorrhoids*.—In examining the anus it will be found that in nine persons out of every ten there are little tabs of withered-looking skin varying in size from a millet-seed to a kidney bean, and of the existence of which the majority of these persons examined were unaware. These are the remains of old external piles, and while they remain in this state are perfectly harmless, but when they become inflamed the pain is very acute, far exceeding any pain ever felt in the ordinary forms of internal piles. They never bleed, and hence are sometimes termed *blind piles*. They become active and painful, not so much from derangements of the portal circulation as from external causes, as for example, sitting, while warm and perspiring, on a stone or other cold seat. I have known cases arise from driving on the outside of a coach on a cold morning. On examination, when in the inflamed state, these external hemorrhoids have the appearance of small glistening tumours, of a pinkish colour, exquisitely painful to the touch, and situated close to the edge of the anus; sometimes partially within it. The speediest relief is obtained by slitting up the tumour and evacuating the clot of blood which it contains, but, as all physicians know, the proposal to treat them in this manner is a harder matter than the more gentle one by medicinal means. In these cases aperients as a rule are positively injurious, and generally speaking are less frequently called for than in cases of internal hemorrhoids. Of all the plans which I have adopted none have yielded such satisfactory results as the following, viz.:—

To bathe the part thoroughly with water, as warm as can be borne, together with the free use of Castile soap, and afterwards to apply equal parts of the compound gall ointment and extract of belladonna. The operation must be repeated every three or four hours till the pain subsides. Usually the first application gives great relief. Without the previous bathing with soap and warm water the application of the ointment is of little service.

2. *Internal Hemorrhoids*.—These are of various kinds—arterial and venous—and are normally situated within the anal ring. They may be found in all stages as regards their formation, size, and the degree of suffering to which they give rise. In every case their origin is connected with some disturbance of the portal circulation, and is not so much affected by external conditions as is the case with the external variety. Unless the tumours are strangulated, or their surfaces ulcerated, they never give rise to the same acuteness of suffering as blind piles. They are also much more amenable to the influence of medicinal measures both internally and locally. The immediate object of treatment in these cases is twofold—

- (a) To relieve congestion of the portal circulation, but more particularly to correct the sluggishness which leads to it; and
- (b) To soothe the irritated parts.

No doubt in a case of well-marked venous internal hemorrhoids some temporary relief may be obtained by the prudent use of the so-called hepatic stimulants—podophyllin, euonymin, and mercury—not from any stimulant effect which they possess over the liver so much as by sweeping away the bile as fast as it flows into the duodenum, and thus preventing its absorption; but sometimes they entirely fail. A second stage may now be said to be reached in the treatment of such cases, and here it is where glycerine appears to be singularly beneficial. The following are given in illustration:—

Case 1.—M. S., æt. 27; phthisical: Has suffered from large internal venous hemorrhoids for upwards of two years. At times they bleed profusely, which relieves her; at other times the distress they occasion is so great that she is obliged to remain in bed for days together. For a week during one of her attacks I examined her motions daily, and found them natural in colour and consistence. She had seldom suffered from constipation. She was ordered a dessert-spoonful of Sargo glycerine twice a day, and in two days experienced great relief. She continued taking the glycerine for two weeks, and after taking it for five or six days the bowels became slightly constipated. The same thing occurred during two subsequent attacks, and was easily counteracted by taking at bedtime a powder composed of sulphur, 10 grains; sulphate of potash, 10 grains.

Case 2.—Mrs. T., æt. 45: Has suffered from internal piles for many years. They used to “come down” upon the least exertion, and otherwise caused her much discomfort. Bowels always regular. Sometimes a second motion at bedtime. She began to take the glycerine last October and soon felt much better. She had less discomfort in the rectum, and could take a fair amount of exercise without their “coming down.” She continued to take the glycerine for ten days before the bowels

appeared to be affected by it. The bowels then became somewhat confined, when she took the powder of sulphur and potash which produced no effect. The dose was increased to 15 grains of each with the desired result. Mrs. T. is now troubled much less frequently than before, and when she is, the glycerine never fails to give her relief.—*Practitioner*, Oct., p. 2500.

44.—ON THE CARBOLIC ACID TREATMENT OF PILES.

At a meeting of the Therapeutical Society of New York, Dr. T. E. SATTERTHWAITE reported having tried carbolic acid injections in three cases of bleeding hemorrhoids, with decidedly bad results. In all of them the tumours became larger and much more painful, and protruded at the sphincter; one case ended in abscess, fistula, and eventually death from phthisis; in the second the patient was lost sight of, but suffered much at first from retention of urine; in the third the usual operation had to be performed a few days after the injection. In the first two cases Dr. S. threw in 8-10m pure carbolic acid; in the third he used 6m acid and 6m glycerine.

At the same meeting Dr. A. H. SMITH related, in detail, two cases of piles, and mentioned shortly six others, in which he had used a *watery solution* of carbolic acid, with the most perfect success. He employs a fine hypodermic needle, the point of which is thrust into the centre of the tumour; ten or twelve drops of 1-20 or 1-30 solution of the acid are then forced in, till the pile is slightly distended. Trifling pain, sometimes only a little smarting, follows, and the patient is usually able to be out and at work the same day. In ten days the piles injected are at most half their former size, when a second operation may be necessary. Dr. S. injects only one or two tumours at a sitting. He makes no reference to the presence of any ulceration or slough, or a hard cicatrix. There are two things that must be specially attended to; the needle must not be inserted too close to the base of the pile, and the solution of the acid must be perfect,—there must be no undissolved globules floating about in it. The conclusion arrived at by the writer is that “the injection of weak carbolic solutions is a means that can be safely employed in completely internal hemorrhoids, or those that are more or less continually protruding from the anus.”—*Glasgow Medical Journal*, April 1879, p. 327.

45.—ON ABDOMINAL SECTION IN INTESTINAL OBSTRUCTION.

By T. R. JESSOP, Esq., F.R.C.S., Surgeon to the Leeds Infirmary. [Several papers on this subject have lately appeared in the *British Medical Journal*. The Leeds surgeons appear to us to

be amongst the first writers on this important question. Mr. Jessop says :]

Is an exploratory operation called for when life is threatened by an acute concealed intestinal block, and if so, under what specific circumstances ?

More than twenty years ago, I assisted in making a *post-mortem* examination of the body of a fine little fellow, the youngest son of one of our most respected citizens, whose disease, rightly and early diagnosed during life, was found to be a simple invagination at the ileo-cæcal junction. The case had run its course with much rapidity ; there was a singular absence of all inflammatory changes, and by the simplest manipulation the gut was restored to its normal position. The conviction was forced upon everyone present, that an opportunity of saving the boy's life had been lost. Similar examples of intussusception are not rare ; I can distinctly call to mind the particulars of three others, in which it was shown on *post-mortem* examination that a reduction might readily have been effected.

Several cases are now recorded in which, by means of gastrotomy, invaginations have been successfully reduced ; and no one I imagine would question the propriety of opening the abdomen in these cases when the diagnosis is assured, and after it has been made clear that other means have failed.

Many years ago, Mr. Seaton related to the members of the Leeds Medical Club the details of a case which, with the one given above, made a lasting impression on my memory.

A vigorous, healthy, and muscular man of thirty was suddenly seized with abdominal pain, vomiting soon followed, the bowels refused to act, and, in spite of appropriate treatment continuously applied, the patient died on the fifth day, with all the symptoms of a strangled gut. On *post-mortem* examination, besides evidences of recent peritonitis in the lower half of the abdomen, a coil of the ileum was found suspended from the front abdominal wall, not far from the umbilicus, by an organised band an inch and a half long. In his remarks on this case, Mr. Seaton writes : "To me it appeared, and still does, that, apart from the danger of opening the abdomen, the division of the band would have cured the patient."

Examples like this, again, will have been met with in the experience of many physicians and surgeons. I have selected the two for special reference out of a considerable number within my recollection, because they afford typical illustrations of conditions recognisable in some measure at least, even at an early stage, and by general acknowledgment admitting of relief from surgical interference.

It is only within late years that I have put the operation of laparotomy to the test, and as yet I have not met with a case of invagination suitable for or requiring surgical aid. I have had the satisfaction, however, of advising in one case in which death was averted by the timely division of a constricting band. It occurred in the practice of Dr. Land of this town, by whom the operation was most ably performed.

The patient was a man aged 45, who, for several weeks prior to his seizure, had suffered daily from abdominal pains and from retching before breakfast. Whilst walking in the street, on the 24th of October, 1875, he was suddenly attacked by most violent pain in the body, so that it was with difficulty he made his way home. Dr. Land was soon in attendance, and by means of fomentations and the subcutaneous injections of morphia he was able to afford some relief to the man's intense suffering. Daily, however, the symptoms of obstructed bowel became more marked:—uncontrollable vomiting, passing from cibal to bilious, and finally becoming distinctly stercoraceous; all escape of fæces and flatus by the rectum absolutely stopped; a gradually increasing distension of the abdomen. On the 31st of October, seven days from the commencement, Dr. Land asked me to share the responsibility with him. Matters had now become grave in the extreme. We both felt that, if unrelieved, the man had not twenty-four hours to live. The progress throughout had so closely resembled that of a strangulated hernia, without the external accompaniments, that we decided to recommend an exploratory operation. This was performed after such delay only as was necessary to obtain assistants and instruments. An incision large enough to admit the hand was made through the linea alba below the umbilicus, with the immediate result of a large protrusion of distended small intestine. After vainly searching the several internal rings, our attention was being given to the ileo-cæcal region, when, on turning aside the intestines, a band thicker than a quill and several inches long was discovered, and beneath it a loop of bowel. Just as the cord was brought from the midst of the distended coils to the surface, stretched over Dr. Land's forefinger, it was seen to give way. The exact attachments of the band were not sought for; there was no bleeding; the visceral protrusion was ever on the increase; there had already been a prolonged exposure, and we were naturally anxious to complete the operation. It probably sprang from a spot corresponding with the base of the sacrum or the last lumbar vertebra and stretched thence to the right brim of the pelvis. The intestines, deeply injected, were replaced, the wound was carefully closed by means of wire sutures, made to include the peritoneum, and the abdomen was covered with cotton wadding and encircled

with flannel. The after-progress of the case was all that could be desired; the symptoms of obstruction at once ceased, and he slowly convalesced. His only inconvenience now arises from a feeling of weakness in the anterior abdominal wall.

It is not, however, in cases of intussusception, which are, generally speaking, easy of diagnosis and readily followed through their various stages, that the surgeon will experience his greatest difficulty in deciding whether or not to interfere, nor is it in cases like Dr. Land's, so closely resembling a strangulated hernia in its symptoms, that he will feel himself most perplexed. There is, indeed, a fair amount of unanimity amongst authors as to the advisability of opening the abdomen in cases both of intussusception and of constriction by bands where the diagnosis is clear. But there are others, more numerous perhaps, of a much more doubtful character, in which the cause of the obstruction can only be determined by an exploratory incision; and it is in these that the question assumes its greatest importance—Is an operation of inspection called for? Are we right, that is to say, in opening the abdomen in an extreme case of bowel obstruction, when the nature of the obstruction is *not* apparent, on the possibility of finding some removable cause? The answer to this question, it appears to me, must depend upon the replies given to two others: What is the general experience of observers as to the causes of fatal obstruction? and are the chances of recovery in any degree lessened by an exploratory incision, where the cause is found to be irremovable by operation?

Mr. Teale answers the latter question by a decided negative.

Mr. Gay, Dr. Hilton Fagge, Dr. Brinton, and others, show that in a large percentage of cases the cause of obstruction is purely mechanical, and, therefore, remediable. Invagination, concealed hernia, constriction of bowel by a band or omentum, volvulus, impaction of concretions or of foreign substances, together constitute, no doubt, a not inconsiderable proportion of acute obstructions; and typical instances of each of these present for the most part sufficiently distinctive features.

What proportion do these and other irremediable conditions bear to the more purely mechanical? Are not cases like Mr. Seaton's and Dr. Land's more common than is imagined?

On the evening of the 21st of October, Mr. Newstead of Leeds called for me to accompany him to a patient's house under the following circumstances. An engine-driver, aged 34, took his train as usual from Leeds to Hull on the 20th of October, arriving at the latter place in the evening. Whilst waiting for his return train to be made up, he was suddenly seized with violent abdominal pain, accompanied by vomiting and faintness. Utterly incapacitated, he was conveyed back to Leeds as a pas-

senger; and when seen by Mr. Newstead at midnight he was still vomiting, in pain, and collapsed. Next day, the symptoms remained unabated; and when I saw him on the evening of the 21st, about twenty-four hours after the commencement of his illness, he was restless, tossing from side to side, vomiting a dark fluid with a stercoraceous odour, moaning continually, and almost pulseless. There was some abdominal tension, but the man was too ill for us to determine the presence or absence of tenderness. He died at eight o'clock on the following morning, about thirty-seven hours after the seizure. We learnt from his wife that he had for some years been subject to attacks of diarrhoea, for which he had several times been compelled to seek medical aid. This information, the suddenness of the attack, together with the extreme and continuous prostration, led us to conclude that an old-standing intestinal ulcer had penetrated the peritoneum, and given rise to faecal extravasation—an opinion we deemed fully warranted by the facts, and confirmed by the rapid termination in death. On *post-mortem* inspection, our diagnosis was not verified. Stretched horizontally across the lower abdomen, the appendix vermiformis was attached by its tip to the sigmoid colon; a knuckle of ileum had become adherent to the posterior and upper surfaces of the displaced appendix; whilst beneath the whole, and tightly held down and constricted, lay a coil of jejunum, black with congestion. The adhesions were readily separated by the finger without the occurrence of any appreciable lesion in the gut; and the neighbouring peritoneum, though congested, was not marked by any inflammatory products. Who can doubt that, had it been possible on Mr. Newstead's first visit correctly to diagnose the state of things discovered in this case after death, the operation of gastrotomy would have offered a very probable means of cure? Are such cases as this and the others I have mentioned more frequent than is generally supposed? I have myself seen several others; my medical friends have related to me many examples. What is the general experience of the profession? If it be in accordance with my own and many of my colleagues and friends, and if I am right in concluding that, under circumstances unfavourable for operation, the exploratory incision, by enabling the surgeon to relieve tension and to cleanse away harmful inflammatory products, is found, on the whole, to prolong life without adding to the sum of suffering, the exploratory incision ought to be more frequently resorted to than heretofore.—*British Medical Journal*, Sept. 27, 1879, p. 488.

46.—TREATMENT OF INTESTINAL OBSTRUCTION.

By Dr. W. H. BROADBENT, Physician to St. Mary's Hospital.

I have recommended and practised puncture of the intestine several times in intestinal obstruction, and have never seen

injurious effects. The precautions which I consider necessary are the following:

1. To secure, if possible, absolute freedom from peristaltic action of the bowel. This is done by giving an extra dose of opium by the mouth, or a considerable hypodermic injection of morphia, or both, three or four hours beforehand. No food of any kind should have been taken for some time.

2. To select, if possible, a coil of intestine which shall contain only gas, and not liquid. This will be in the jejunum, and is to be found above the umbilicus rather than below it. An indispensable condition is, that scarcely any food shall have been taken during the entire attack.

3. To pierce the coil exactly at its most convex part. The abdomen should be carefully watched for some time at every visit, and especially before the operation. In some cases, where the walls are thin, the outlines of various coils may be traced even in repose; but this will be more distinct when peristalsis is provoked by pressure, friction, or manipulation of one kind or another; it will be seen also which coils shift and which keep the same position when contracting. The spot chosen for the puncture should be as nearly as possible over the centre of a coil which does not roll about, and by preference in the linea alba. If the needle happen to hit the line of contact between two coils, it may tear both.

4. To exercise great care and patience during the escape of the gas. The needle should be held lightly, but rather firmly, perpendicular to the abdominal wall, and should not be allowed to follow too readily any movement of the intestine. Under the circumstances of obstruction, the respiratory movements are not great. As the gas escapes from the coil selected for puncture, it will collapse under pressure from neighbouring coils, and the flow through the needle will cease; very soon, however, the air in the intestine will distribute itself and enter the empty portion, when it will again escape. This may be aided by gentle manipulation and pressure; but they should not be hastily resorted to: nothing is gained by hurry. Should the tube get blocked, aspiration may free it; but it is safer to drive a little air through the tube into the bowel than to exert powerful suction, which may draw the mucous membrane against the sharp needle.

It is better not to put on a bandage after the operation.

Puncture of the intestine can relieve obstruction only very rarely, and under exceptional circumstances. In one case in which there was reason to suppose that the cause of obstruction was external to the bowel, and was due to pressure by the tumour, or by adhesion, or to displacement and dragging of a

portion of intestine, it was hoped that removal of distension might permit the parts to return to their previous condition and situation; and other conditions may be imagined in which this might occur, but it could have no effect on a stricture or intussusception. My own experience, however, would lead me to recommend puncture as a palliative; and, though I have no experience to guide me, I should think it might be an useful preliminary to inflation, manipulation, suspension head downwards, or other procedures in intussusception, twisting, or imprisonment of the bowel by adhesions.—*British Medical Journal*, Sep. 27, 1879, p. 490.

47.—TREATMENT OF INTESTINAL OBSTRUCTION.

By W. H. A. JACOBSON, Esq., M.B., Surgeon to Guy's Hospital.

In certain cases of acute intestinal obstruction, where previous treatment has entirely failed, to open the abdomen is not only justifiable, but imperatively indicated. I know how fatal such operations have been; but I know also that very rarely (I speak of *acute* intestinal obstruction) a case gets well if left to itself. To some, the very recommendation of an operation at all may savour of that rashness which comes of inexperience. I would reply that the very want of success in previous operations is accounted for by the fact that they were too often not performed under those conditions which I am about to point out as being essential to success.

Very briefly, these conditions are the following:—1. The operation of opening the abdomen is only to be performed where certain previous treatment has failed. 2. It is only to be performed antiseptically, so that the risk of septic peritonitis may not be added to the list of dangers against which the patient has already to struggle. 3. But, above all, the operation must be performed earlier than has heretofore been the case. It is hopeless to perform it when symptoms of peritonitis or enteritis have set in. Those who wait, as I venture to say too many have waited, till the abdomen is generally and enormously tympanitic—till the temperature is persistently high, or is only falling before the inevitable end—till the pulse is running down and the patient in a condition of irrecoverable collapse; those who wait till any or all of these conditions are present, had far best not operate at all. It is only too easy to foretell the operation that follows. The abdominal cavity is opened readily enough, and then the difficulties begin; coils of enormously distended intestine at once crowd up at the mouth of the incision; the operator has the greatest difficulty in finding a contracted portion, and so of tracing out the point of obstruction; perhaps, during his endeavour to do so, a

fæcal odour becomes apparent, showing that the intestine has already given way; or, granting that the seat of mischief is found, and the cause of strangulation removed, in spite of all his endeavours the surgeon finds himself with several coils of intestine outside the abdomen, and is at his wits' end to get them in again; perhaps he punctures them with the aspirator-needle; but, owing to the paralysis of the muscular and the infiltration of the cellular coat, which has by this time taken place, the puncture in the mucous coat is not closed, or does not slip away from directly beneath the opening in the serous covering; as the collapsed intestine is returned, liquid fæces are seen escaping at three or four points; any attempt to close these points with sutures only makes matters worse, and the patient sinks quickly after his removal from the table.

I have already pointed out the conditions which would guide us as to the time at which an operation, to be successful, must be performed. I will now mention a few points to be observed during the operation itself. The strength of the solution to be used for spray and sponge must not be above 1 in 40; the solution must be duly warmed. If the operator be obliged to draw any coils out of the abdomen for the purpose of examination, the spray must not be allowed to play upon these coils, which are to be covered with carbolised lint, taking care that the smooth and not the fluffy surface of the lint is in contact with the intestine. If any amount of carbolised solution be believed to have entered the peritoneal cavity, this is to be removed by careful sponging; and finally, in closing the wound, catgut being unreliable, twisted silver sutures are to be employed.

In conclusion, I venture to suggest the following points in the treatment of a case of acute intestinal obstruction.

1. From the very first, it is not enough to refrain from giving solid food. The patient is to be fed by enemata, given *per rectum*, and consisting of milk and egg, wine or brandy, and given carefully twice or three times daily. If possible, only ice out of milk is to be sucked by the mouth.

2. If possible, no opium is to be given, as this drug has a dangerous tendency to mask symptoms; instead of opium, belladonna is to be given, and to be pushed in large doses. Many of those present may remember a paper in the British Medical Journal (August 31, 1878), by Dr. Norman Kerr, on "Large Doses of Belladonna in Intestinal Obstruction." Five cases are given, somewhat briefly, and therefore incompletely recorded; all got well under the use of belladonna, though in three acute cases, two grains were given every hour, and sixteen, twelve, and fourteen grains were administered in these cases. I do not pretend to explain the benefit of this drug. I admit

that its action would seem somewhat contradictory in the one case, as when rubbed into the perineum for spasmodic stricture, appearing to remove spasm ; in another, to set up contraction of involuntary muscular fibre. But in a case like the one which I bring before your notice, where a coil has, perhaps by irregular action, slipped under one or more bands, I can imagine that belladonna, if given early and in large doses, may enable the bowel, if this remain empty, by its own contraction, to set itself free once more. But on this point I should be grateful for any information.

3. At an early date, the method of abdominal taxis should be made use of. Mr. Hutchinson, in bringing this method forward, recommends that it should be carried out as follows. Under chloroform, a very copious enema of water is to be given by a long tube, and, the anus being kept closed round the tube with a cloth, the fluid should be forced in to the utmost point of distension. Then, simultaneously with the withdrawal of the tube and the escape of the water, the surgeon, with the flat of one hand on each side of the abdomen, should press gently but firmly on alternate sides, in such a way as to facilitate the movements of the coils upon each other. As much of this having been done as shall seem advisable, and the water having flowed out, let the patient (by means of a girth fastened to the bedposts) be raised by the feet till the trunk is inverted ; and, whilst the patient is in this position, let the surgeon, with both his hands placed on the lowest part of the abdomen, press the whole mass of the intestines as high up as possible in the abdomen. Of course, during the whole of the time an assistant should carefully watch the effects of the chloroform on the pulse and breathing.

I have only one small alteration to suggest in the above details ; and that is, knowing the difficulty of inverting a heavy patient, I think it will be found better to overturn a chair, so that its sloping back be against the bed ; over the chair-back let some clothes be laid, and the patient's body be gradually inverted over this by assistants standing on the bed. This will not only be found easier in the case of a heavy patient, but in that of a female one more decent in the eyes of any friends who may be present.

The above methods failing, I have only to urge an early operation. It may be said, in answer to this, that recovery may and does ensue sufficiently often to make it worth while to wait. From the cases that I have seen, my own opinion would have been distinctly to the contrary—that, apart from those chronic or subacute instances where a case does well by an intussusception slipping back again or sloughing off ; where a long-impacted scybalus or gall-stone passes on, or where a

stricture is set free by partial ulceration of itself—that, putting aside such cases as these, acute intestinal obstructions, which have not yielded to such preliminary treatment as that sketched above, will end but in one way if left to themselves. No doubt the justifiability of these operations depends on the probability of a successful result; but hitherto, in nearly every case, the operation has been performed too late to guarantee any such success. For my own part, considering what has been attempted and what has been done in antiseptic surgery, and believing that great advances have yet to be made in the surgery of the abdomen, I trust that I do not assume too much when I say that, in a few years, we shall be able, in cases similar to that brought before you to-night, to promise a successful result with something like certainty.—*British Medical Journal*, Sept. 27, 1879, p. 493.

48.—TREATMENT OF INTESTINAL OBSTRUCTION.

By T. PRIDGIN TEALE, M.A., Surgeon to the Leeds Infirmary. [Some remarkable cases were published by Mr. Teale a short time ago, which were commented upon by Mr. Bradley, of Manchester. We think Mr. Teale had decidedly the better argument, and showed some very good cases to warrant his practice of abdominal incision. The great fear on the part of most surgeons would be peritonitis. Mr. Teale says]

The object of my paper was to reduce the dread of peritonitis to what I believe to be a more true position in the treatment of intestinal obstruction, and to maintain, that in those cases in which recovery seems hopeless, unless surgical relief can be given, we need not be deterred by the fear of peritonitis from submitting the patient to “the safer risk” of exploration.

I claim that as far as they go my cases support this object. Six are recorded. Three of the patients proved to be so exhausted as to be in a hopeless state. In one there was a burst bowel; in a second an abscess had burst amongst the intestines; in the third the intestines were obstructed by and studded with cancer. In none of these was there any question of setting up peritonitis. They form no argument, *quod* peritoneum, either for or against exploration. A chance of recovery was neither given nor taken away.

Let us examine the three remaining cases. In all there was a healthy peritoneum. In all the nature and seat of the obstruction was unknown. One recovered completely. A second was free from peritonitis for nine days, until in a fit of coughing the abdominal wound was burst open, an accident which would in all probability not have happened had my attempt to relieve the obstruction by colotomy been successful.

The third had no sign of peritonitis as a result of the operation, but died on the ninth day from bursting of the bowel and escape of fæces into the peritoneal cavity. In none of these cases did peritonitis prove the rock on which the case was wrecked.

It must needs be that, in the present state of "peritoneal surgery," exploration of the abdomen should be reserved, and rightly, for the most hopeless cases of intestinal obstruction, such as were all those which I have reported. Many will prove to have been hopeless from the beginning, some will prove to have been submitted to operation too late, a few will recover as a direct result of the operation. As we gain experience and confidence in our means of diagnosis and treatment, we shall rescue from death some of the cases now lost by delay. Is it too much to hope that we are now marching by means of failure onward to success in "peritoneal surgery"? May we not take a lesson from the position of ovariectomy of the present as compared with ovariectomy in the past?

One word more. Case No. VI in my paper, seems to point the way to dealing with distension of the intestines, one of the most formidable and dangerous factors in obstruction of the bowel. If further experience should confirm the teaching of this case, that we can effectually empty of gas and fluid distended intestines, and that intestines so emptied do not rapidly, if ever, regain their state of distension, we have therein the key to the successful treatment of an important class of fatal illnesses. —*British Medical Journal*, Sept. 27, 1879, p. 494.

ORGANS OF URINE AND GENERATION.

49.—ON THE PRODUCTION OF CYSTITIS BY CONTAGION THROUGH THE USE OF INSTRUMENTS.

By Sir HENRY THOMPSON, Bart., F.R.C.S., &c.

I have long suspected that cystitis is capable of being propagated by the direct transference of inflammatory products from the bladder of one patient to that of another. All are sufficiently familiar with the fact that purulent matter from the vagina, and probably from the uterus also, produces inflammation of the male urethra, and that conjunctivitis may be caused by contact with pus from either source; and I believe it is quite unnecessary to imagine that any specific quality attaches to purulent matter produced in these localities, rendering it more than ordinarily virulent and contagious. Certainly no proof can be adduced that such quality exists; a decision on this point, however, does not necessarily affect the question whether cystitis may be originated or not by contagion.

Every one knows that the operation of sounding the blad-

der—it may be for stone or for tumour, etc.—is sometimes, although rarely, followed by an attack of inflammation more or less severe. Such an occurrence is, in some circumstances, not unnatural. A delicate organ is mechanically disturbed, and, if force be employed in the process, some inflammation of the mucous membrane is a not improbable result. Hence the extreme importance of adopting a method and instruments which shall accomplish the object in view with the smallest degree of distension and movement; and also of forbearing to make such an exploration, except in circumstances which manifestly indicate its necessity. In my experience of such cases of this kind as have fallen under my observation during many years, I have remarked that the inflammatory attacks which follow sounding occur in two modes, distinct from each other. Thus, in some instances, the patient has a shiver, occurring within three to four hours of the time of the examination; soon afterwards, the urine is passed too frequently and with pain, becomes cloudy, and some general fever sets in. In such, the cause of inflammation is clearly a mechanical one, and, if the patient be healthy, it soon subsides with rest and treatment. But, in a few other instances, no disturbance occurs until the lapse of forty to fifty hours, or thereabout, after the sounding. The subject of the examination has been in all respects well since the sounding took place, and felt, if anything, only slight soreness during the first few hours following the operation. After the interval named, he experiences a little undue frequency of micturition, loses appetite, is chilly or has a shiver; and by degrees symptoms of cystitis appear, and continue a marked course for a few days, with varying persistence according to circumstances. Usually, the patient attributes his condition “to some cold he must have caught the day after the examination,” and by no means attributes his troubles to the instrument, as he infallibly does in the circumstances first described.

Why, in certain circumstances, these phenomena should occur so long after the provocation which must have given rise to them, has, as I have already intimated, frequently afforded me an interesting subject of speculation. But a case has recently occurred, which I have been enabled to watch closely, and which seems to throw light on the nature of these examples of the second kind. I shall give the chief particulars in detail.

A medical man, under sixty years of age, having had occasion, as he thought, to pass for himself a silver catheter (No. 10) daily, had a new one made; there was a peculiarity in its construction, the lower or curved portion, about two inches and a half in length, being separate and attached by a screw to the shaft. Such catheters were frequently made formerly for the

purpose of packing in a surgical pocket-case. He passed this daily with great ease during some weeks, on no occasion producing irritation. One day, and this was the only occasion on which he used the catheter for another person, he introduced it into the bladder of a patient whose urine was highly mucopurulent, and who was indeed suffering with severe cystitis. He believes that, immediately after using the catheter, he washed it in the ordinary way. Subsequently, on that day, he employed it as usual for himself; and it is somewhat curious that he did not use it the next day—not because he felt any irritation, but, on the contrary, because he was arriving at the conclusion that the instrument was no longer necessary. The next day but one after his last employment of the catheter, about forty-four hours after, he felt chilly, and micturition was slightly painful. Next day, he had some fever, no rigor, but increase of temperature; his urine was cloudy and passed frequently. The day after, he was confined to bed; the temperature varied between 102 deg. and 103 deg. for a few days, and the urine was loaded with mucopurulent jelly-like deposit during one or two days. After more than a week's confinement to his room, he gradually improved and soon perfectly recovered, having in his urine now no trace of the attack; he empties his bladder perfectly, and, in relation to the urinary system, has nothing whatever to complain of.

The circumstances of this case will go far, I think, to suggest the strong probability that this attack of cystitis was caused by the transference of infectious matter, by means of the catheter, from the patient for whom it was once used to the subject of our case. I can scarcely doubt that the exceptional formation of the instrument, the screw-attachment, which on examination, moreover, appeared to be a little loose, offered a chink, in which matter lodged, especially as this lower part was not detached for cleaning—the eyes of the catheter serving that purpose, as in the ordinary instrument.

It may very naturally be urged: If inflammation be so easily produced through contagion by passing instruments not scrupulously rendered clean, so numerous and varied as these are, and so frequently used, how is it that cystitis is not a very frequent result—for this it certainly is not—of ordinary catheterism?

I think the reason is not far distant, and that it may be found in the action of the catheter itself. The moment the instrument reaches the bladder, the urine rushes through the orifice, and carries off in its current any minute particles which may be adherent to its extremity. In bougies, no opening for the lodgment of adventitious matters exists, and any risk of contagion by their use must be considerably less. Besides, the action of the urethra itself, clinging to the instrument and

sweeping off, almost at the external meatus, as it does by that action, most of the lubricating material, is a sort of defence to the internal passages from danger. On the other hand, in examining a bladder, the sound is rarely used as a catheter, and although it has often an eye in its extremity, the handle is closed, and urine seldom passes through it. The various movements of a sound in searching the bladder are calculated to detach, within its cavity, foreign particles, if any such exist, in or about the eye.

The practical question, how to prevent any transference of matter to the bladder and urethra, in employing instruments of any and every kind, presses for solution. It is one of extreme importance to all concerned, and the occurrence of an accident of the kind described, however rare it may be, is one the bare possibility of which cannot be contemplated without extreme repugnance.

After some consideration and some experimental trials, I think the following recommendations will render contagion by instruments impossible.

Firstly: All metal instruments—catheters, sounds, and lithotrites—after use, at any rate in cases of muco-purulent urine, should be plunged for a minute or two into boiling water, to which either a little common soda or a little carbolic acid has been added. If the boiling point of water be not considered absolutely sufficient, a strong solution of chloride of zinc in water may be used. At the strength of 12 per cent. solution, the boiling-point is 220 deg. Fahr., or eight above that of boiling water. For some years past, as advised in the last edition of my lectures, I have always placed all gum and other catheters and bougies in a bath of weak carbolic acid immediately after use.

Secondly: I have more recently—that is, since the occurrence described—added a solution of carbolic acid to the oil used for the lubrication of instruments. Oil being the remedial agent for the caustic effects of carbolic acid, there is no danger in applying to the urethra a comparatively strong solution of the acid in oil, since no irritating effect whatever is produced, and the disinfectant influence is unimpaired.

For the last two months I have used the following formula, and can, therefore, guarantee that it is absolutely unirritating: *Rx* Acidi carbolici med. gr. xii.; olei olivæ ꝑi.

A free use of this as a lubricant to all instruments before using will, I believe, ensure, at all events in combination with the modes of cleaning just described, safety from the occurrence of any contagion by means of instrumental treatment.—*British Medical Journal*, May 10, 1879, p. 694.

SYPHILITIC AFFECTIONS.

50.—ON THE TREATMENT OF SYPHILIS WITHOUT MERCURY.

By Dr. J. P. H. BOILEAU, B.A., F.R.C.S.I., Surgeon-Major, Assistant-Professor of Pathology, Army Medical School, Netley.

It is believed by not a few, that no wise man would attempt to dispute the efficacy of mercury as an antidote to syphilis. Many hold that the disease cannot be eradicated from the system without the use of the mineral. With my experience, I cannot but hesitate to accept such views.

My position is briefly this. In May 1865, I was gazetted to a regiment about to proceed abroad. I served with it in the Mediterranean, in Canada, and in the West Indies, and at home; and left it in October 1876. For ten years of that period, I was most closely associated with the regiment, and had many facilities for prosecuting inquiries into the *results of the treatment of disease, and, by observations, continued year after year, for satisfying myself as to the state of my patients, long after they had been the subjects of my treatment*; and so I am in a position to-day to bring to your notice, not only cases of syphilis treated without mercury, but, I believe, cases of syphilis cured (by natural processes, or otherwise) without mercury.

The principle which in the main guided me in the treatment of syphilis, was the principle of avoiding mercury as much as possible; and, in carrying out this principle, I treated most of the cases without prescribing any form of that remedy. Rarely did I resort to fumigation or inunction; and I now believe that it would have been better had I discarded all local remedies having mercury in their composition. As a rule, I did not use such applications. I tried the internal administration of mercury in some cases, without being at all satisfied concerning its alleged antidotal or curative powers.

My general principle is well attested by a gentleman (now a commissioned officer, and occupying one of the most responsible positions in connection with the Army Hospital Corps) who for eight years as hospital sergeant and compounder carried out my instructions. In reply to questions which I have sent him, he writes: "As regards your treatment of syphilis, I know you were always averse to ordering mercury for the disease; indeed, I am quite certain of this, as you invariably marked the venereal register—NO MERCURY." And he adds: "As well as I can recollect now, all the men treated by you for the disease made good recoveries. And I don't remember a single man who was invalided, excepting L."

Such are the indisputable facts of the case. For several years, I treated syphilis, in the majority of cases, without the use of

mercury internally or externally ; and in all such cases, without any exception, the ulterior results were most highly satisfactory ; even the man L., who was invalided, returned to his duty in good health, and served for some years afterwards.

The following cases illustrate the general mode of treatment, and its results.

Case 1.—A. M., aged 32, had a solitary sore in grizzly induration. The occipital and inguinal glands were much enlarged. He had roseola and rheumatoid pains. He was treated in September 1867, locally, by fuming nitric acid, and water-dressing. The chancre healed in twenty-three days. Internally, he had iodide of potassium, nitrate of potash, compound ipecacuanha powder, and liquor opii sedativus. In February 1868, he was looking well, and at duty. In March 1871, he was in excellent health ; and he continued so until he was discharged the service.

Case 2.—B. T., aged 20, had an indurated sore, roseolar and papular rash, psoriasis of the arms, and very severe ulceration of the tonsils. He was treated October 1867, locally, by nitrate of silver and sulphate of copper. Internally, he had iodide of potassium, cinchona, and compound ipecacuanha powder. In May 1868, he was in excellent health, and remained in good health until transferred to another regiment in February 1873. I heard in 1877 that he was in good health.

Case 3.—C. J., aged 25, had an indurated sore, indurated glands in both groins, and roseolar and papular eruption, very marked and widely spread. He was treated in December 1867. He was in very good health when he took his discharge in 1871, and also by accounts lately received. The only local application was cold water ; and the chancre healed readily, although the induration was very persistent. Iodide of potassium was administered in decoctum cinchonæ, and he had the usual hot baths.

I must state that all my cases of syphilis were treated by hot baths. Whatever variety there may have been in the use of other remedies, there was none in this. *Hot baths was the standing order for all cases of syphilis.* And, for the most part, the men were strictly confined to bed, often notwithstanding their entreaties to be allowed to get up. The most scrupulous attention was paid to the cleanliness of their linen.

Case 4.—D. W., aged 25, seven weeks previously to his admission for true Hunterian chancre, was under treatment for phagedenic ulceration of the prepuce. With the hard sore, he had indurated inguinal glands. The initiatory fever of secondaries soon appeared, and with it a dusky purplish papular rash, having a pustular tendency. The nates became covered with condylomata, and the scrotum before long looked like a piece of raw

beef, dotted with little patches of oyster-shell. He was treated in December 1867, locally, by sulphate of copper. Internally, he had iodide of potassium, cinchona, and Dover's powder. In May 1868, his temperature was 98°, tongue clean, skin perfectly clear; there was not a trace of any rash, pimple, or sore. The scrotum was perfectly healthy looking. He was in robust health, and he continued so until he deserted some years afterwards.

Case 5.—E. D., aged 25, had a circular indurated ulcer, enlarged and indurated glands in the right groin, roseola, and a papular rash. He was treated in July 1868, locally, with cold water only by day; and by night, unguentum simplex. Internally, he took chlorate of potash, quinine, and Dover's powder. In October 1868, he was in good health. In March 1871, he was in excellent health. In 1878, he was in good health, and still in the service.

Case 6.—F. G., aged 26, was twenty-six days in hospital for primary sore, with inflamed glands, which did not suppurate. He had a papular eruption over the whole body, and ulceration of the tonsils. He lost all the hair on his head and face. He was treated in September, 1868. In May 1869, there was no trace whatever of the disease. He was in good health when discharged the service in 1871; his hair had all grown again. Chlorate of potash and Dover's powder were the remedies employed.

Case 7.—G. C., aged 20, had Hunterian chancre, with indurated glands on both sides, and roseolar and papular rash. He was treated, in August 1868, for phimosis, complicated to such an extent that circumcision was necessitated. Alum and carbolic acid were used locally, and iodide of potassium and Dover's powder internally. In December 1868, his health was good. In June 1869, he had not a trace of the disease about him. In March 1871, he was in excellent health. There was a very fine white cicatricial line on the prepuce.

Case 8.—H. C., aged 26, was treated in November 1869. He was in good health in Jamaica in 1871. In 1878, I was informed that he was married and the father of two healthy children.

Case 9.—I. J., aged 22, was treated in December 1869. In January 1871, his health was fair. In May 1871, his health was good. In 1878, he was in good health, and the father of two healthy children.

Case 10.—J. R., aged 19, was treated in January 1869. In January 1870, his health was good. In March 1871, he was in good health, and was so when discharged the service.

The above are examples of cases treated without any mercury. Many others of a similar character passed through my hands;

but it is my desire that not only such, but examples of a number in which mercury was locally applied, should be given; because my position is sustained not only by several individual cases such as those narrated, but by the very satisfactory eventual results of all the cases treated by me between 1865 and 1876; and, in order that I may be justified in bringing forward this *tout ensemble* of my practice as a proof of the curability of syphilis without mercury, I am bound to give examples of those cases in which the mineral was locally applied.

Case 11.—N. G., aged 26, had extensive ulceration in, and surrounded by, cartilaginous induration. The inguinal glands were enlarged. He had roseola and pharyngeal ulceration. *Lotio nigra* was used in this case. He was treated in October 1868. In March 1869, he was in good health. In April 1870, he had remittent fever, but was discharged in good health in May. In March 1871, he was in excellent health; and in 1877 was still in the service and in good health.

Case 12.—O. C., aged 23, had a circular solitary indurated ulcer, enlarged inguinal glands, roseolar and papular rash over the whole body, and mucous tubercles on the inside of the thighs; the scrotum was covered by an eruption of papules. On several parts of the body, small gangrenous ulcers appeared; and he had pharyngeal ulceration. The only mercurial preparation used was *lotio nigra*, and that only for a period not exceeding seven days. He was treated in October 1867. In March 1868, he was in fair health. In May 1868, he looked well and felt so. Not a trace of disease was manifest, excepting some slight coppery patches on the inside of the thighs. In May 1871, he was in excellent health, and was in good health when discharged in 1874.

Case 13.—P. M., aged 25, had indurated sores, indurated glands in the right groin, and roseola. *Lotio nigra* was used for a few days, and the only other drug employed was spirit of nitrous ether in camphor mixture. He was treated in February 1866. In May 1866, he had scarcely a trace of disease about him. He was in good health when he deserted in 1869.

Case 14.—Q. L., aged 26, had an excavated hard chancre, four enlarged glands in the left groin, a roseolar and papular eruption, and condylomata of the nates. *Lotio nigra* was used for a few days, when water-dressing was substituted. He was treated in April 1868, and made a rapid recovery. In June 1869, he was in vigorous health, with no evidence whatever of disease about him. In March 1871, he was in excellent health. In 1878, he was in the service and in good health.

Case 15.—R. F., aged 23, had a chancre in great cartilaginous induration; the inguinal glands on both sides were enlarged

and hard. He had an eruption over the whole body of a roseolar, papular, and squamous character; and tonsillar ulceration. *Lotio nigra* was used for a few days. He was treated in February 1870. In June 1870, he was in good health. In September 1871, he was in very good health. He was in good health when discharged the service in 1876.

Case 16.—S. N., aged 22, had a circular hard sore, indurated glands, and macular rash. *Lotio nigra* was used. He was treated in October 1871. In 1876, he was in good health, married, and a father of one healthy child.

Case 17.—T. J., aged 21, had solitary sore, enlarged glands, roseola, pharyngeal ulceration, and condylomata of the nates, scrotum, and thighs. The only mercurial preparation used was the milder ointment of nitrate of mercury occasionally at night. He was treated in January 1868. In May 1868, he was much improved in health. In August 1868, he had not a trace of disease about him. In March 1871, he was in excellent health. In 1877, he had no trace of disease; his health was good.

Such local applications as black wash, calomel, red precipitate, or citrine ointment do not in the least degree, in my opinion, diminish the value of the cases in establishing the fact that syphilis can be cured without the specific action of mercury. In my cases, the actual amount of mercury imbibed by the system must have been very small indeed; the remedies were not at all pushed, quite the contrary; and anyone who could attribute to their use the good results which followed must be a believer in the Hahnemannian doctrine of infinitesimal doses. It must be remembered that advocates of mercury contend that, without approaching the direful effects of salivation, a cure can only be effected by introducing a considerable quantity of the mercury into the system, and by protracting its use over a long period.

Those cases of syphilis in which I have used mercurial preparations locally have, with great complacency, been quoted against me, as mere illustrations of cases cured by diaphoretics, alteratives, and small doses of mercury. I admit nothing of the kind. When I reflect on the composition of these local applications, and on the quantity likely to have been absorbed into the system, I say it is a perversion of the facts to attribute the recoveries in such cases to small doses of mercury. I have, however, treated too many cases of syphilis without any mercurial preparation whatever to doubt that the disease may be cured without them. My sheet-anchor has in all cases been the frequent employment of hot-water baths; the patients being a good deal confined to bed, the most scrupulous attention being paid to personal and surrounding cleanliness, full

hospital diet always allowed, and with some exceptions, a pint bottle of the best ale daily. Iodide of potassium took second place in the treatment, and other salts of the same alkali were freely used at times.

The majority of the patients were young men, and when they contracted the disease were in good health.

During their treatment, they were the subjects of rigorous discipline.

For the purpose of illustration, I have chosen from the hundreds of cases of venereal disease which I have treated only examples of those in which characteristic secondary manifestations followed indurated chancres without suppurating buboes. I thus silence those who might contend that perhaps my cases were not cases of syphilis at all.

I would beg of those who hold that the specific action of mercury is absolutely necessary for the eradication of syphilis from the organism to consult the works of Hughes Bennett, Lancereaux, etc., for a list of observers, committees, or councils that have declared in favour of the simple treatment of syphilis.

My experience as an army medical officer of fifteen years' service, ten of which were passed with one regiment, compels me to believe that syphilis is as curable without mercury as is small-pox or typhoid fever. And until I have some better proof than a mere *ipse dixit* that all the patients that I have treated without mercury are still under the influence of the syphilitic poison, I certainly cannot admit it. To the logic, however, of well-sustained facts, I am quite vulnerable.—*British Medical Journal*, July 19, 1879, p. 82.

51.—ON THE TREATMENT OF UNHEALTHY LOCAL AND SYPHILITIC SORES BY IMMERSION.

By ARTHUR COOPER, Esq., late Surgeon to the London
Lock Hospital.

The method of treating phagedænic and other ulcers by more or less prolonged immersion of the patient in warm water is by no means new, having been employed for many years by Hebra in syphilitic and other diseases of the skin; while in this country Mr. Hutchinson, who, I believe, introduced it, has repeatedly called attention to its value. Mr. Coulson also has published cases treated in this way. Dr. Simmons, of Yokohama, and Dr. R. W. Taylor, of New York, have likewise written in favour of the plan. Weisflog makes use of a "faradising" bath as follows:—One of the electrodes is connected with the bottom of the bath, and as soon as the wound is submerged in the warm water the patient touches the other electrode, which is covered with sponge, with the tip of one finger, gradually bringing the

other fingers into contact with the sponge, according to the sensations he experiences in the ulcer. Pain is said to cease almost immediately. Mr. Hutchinson combines irrigation with immersion, the patient being supplied with an irrigator, by means of which a forcible stream of water is to be directed on the sore while he sits in the bath. In France M. Hémard has for more than twenty years been in the habit of treating chancres by irrigation with cold water for one minute seven or eight times a day.

The following is the plan of treatment which was followed at the Male Lock Hospital during the time I was house surgeon there:—The patient sits in an ordinary hip-bath, containing sufficient water to ensure constant submersion of the affected part, for eight or ten hours a day. If the disease be in the groin, a full-sized bath, in which the patient can recline, may be necessary. The temperature of the water is regulated by means of a thermometer, and is kept as near 98° F. as possible by the removal and addition of small quantities of water at frequent intervals, without disturbing the position of the patient, and in winter the bath is placed near a fire. The exposed parts of the body are covered with blankets. In the evening, finely-powdered iodoform or other dressing is applied, and the patient goes to bed as usual. Next morning the dressing is allowed to separate in the bath, the pain attending its removal being thus avoided. The bath is repeated day after day as long as may be necessary, general treatment, according to the nature of the sore, being carried on at the same time. A good purge is beneficial at the outset, followed by iron, quinine, or ammonia and bark with opium, in local sores, and by appropriate specific remedies when the patient is syphilitic. A liberal diet with plenty of milk, and little or no alcohol, answers best as a rule. Though men only are here referred to, it need hardly be said that the same treatment is equally applicable to women. A severe case of phagedæna in a female forms the subject of Dr. Simmons's communication already noticed.

I have notes of thirty-one cases treated as above during 1876 and part of 1877. They were under the care of Mr. Alfred Cooper and of Mr. Milner, to whom I am indebted for permission to publish them. These thirty-one cases include twenty-two of phagedænic or sloughing sores of the prepuce or glans penis or both, of from a few days to several weeks' duration; two cases of sloughing after circumcision; one case of slowly extending phagedænic buboes in a non-syphilitic subject; one case of acute phagedæna of the sheath of the penis, in a patient with syphilis of nine months' duration; one case of tertiary ulceration of the glands; one case of tertiary ulceration involving the whole penis and part of the scrotum; one case of slow phage-

dænic ulcers of the prepuce and thighs; a case of very rapid sloughing phagedæna of chancre and bubo; and a case of gangrene of nearly the whole body of the penis.

As the limits of this paper will not allow of my giving the cases in detail, I shall only briefly notice their leading features. The shortest period during which the bath was made use of was one day; the longest was twelve days. Most of the twenty-two comparatively recent cases of phagedæna or sloughing attacking a chancre, or the initial lesion of syphilis, became healthy in from two to six days, and a few slight ones became so after one day. The most severe case was that of a strong healthy-looking man, aged twenty-one, in whom a local sore on the penis and a bubo in the left groin were attacked by sloughing phagedæna, which extended so rapidly that in a few days the cavity in the groin had the diameter of one's fist. Here the sloughing ceased to extend after four days, but the bath was continued for eight days afterwards. Both chancre and bubo then rapidly healed. The case in which nearly the whole of the penis was lost was that of a man of broken-down constitution and drunken habits, aged fifty-seven. No satisfactory history could be obtained beyond the fact that a sore had appeared about two days after connexion, a month before admission into the hospital, by which time about two-thirds of the penis had sloughed. There was no extension after one day in the bath, and after four days the whole of the slough came away. In three weeks from the time of admission the patient went out with a stump about an inch long and nearly healed. There were no signs of syphilis.

In three cases morbid action reappeared, the ulcers having been healthy when the bath was discontinued. Perhaps in these it was left off too soon, or continuous immersion day and night might have succeeded. However, one of them got quite well eventually under other treatment. One was lost sight of after attending for some time as an out-patient. In the third the parts were quite healthy and healing when he was discharged, after having been three months in the hospital.

In three instances failure resulted. The first of these was a case of malignant syphilis with a very large sloughing primary sore, the patient being at the same time almost covered with an ulcerating syphilide. After nine days of the bath he refused to continue it. In the second case failure was due to the man being too stout to sit comfortably in the bath. The third and most complete failure was in a man who had suffered very severely from tertiary syphilis, and in whom the whole of the penis and part of the scrotum were involved in one large unhealthy ulceration. The pain was intense, and the patient said the bath aggravated it. The parts, after some time, healed

entirely under large doses of iodide of potassium and opium and the local application of thick layers of cotton wool. This is the only case I have seen where pain was not quickly relieved by immersion. Mr. Coulson has recorded an instance in which the bath failed, although continued for eight successive days and nights, as well as during thirteen days, the patient being allowed to go to bed during the night. Mr. Hutchinson also mentions an unsuccessful case.

In order to obtain the full benefit from immersion it is necessary to bear in mind a few practical points. First, care must be taken to see that the affected part is thoroughly and constantly submerged; the penis has a tendency to float, and if the supply of water be too scanty, and the extremity of the organ be the seat of the ulcer, this may be altogether above the surface, and thus failure will occur. It is also imperative that the whole of the unhealthy part be exposed to the action of the water; if phimosis prevent this the foreskin should either be slit up or removed altogether. When the prepuce is simply divided the edges of the wound always swell more or less, and sometimes so much that the glans becomes almost buried, the discharge does not get freely away, and the length of time necessary for immersion is prolonged; besides, a subsequent operation to remedy the deformity is generally necessary. Two lateral incisions, as recommended by Dr. R. W. Taylor, answer very well in completely exposing the parts, but here, also, a second operation is required for the removal of the flaps. Circumcision gives the best results, and was practised in several of the above cases, and as the wound, under immersion, scarcely ever takes on the diseased action, it may usually be done without fear. In case of troublesome hemorrhage, either after operation or otherwise, solution of persulphate of iron may be applied, a bandage put on, and the bath deferred until the following day. Protection of prominent parts of the patient's body should not be forgotten. A few folds of an old blanket for him to sit on, and the same behind his back, answer very well in a hospital; but in private practice air or water-cushions may be used. Dr. Simmons recommends a large sponge for the patient to sit on. The bath should invariably be continued for at least a day after the ulcerated surface has become quite healthy, though it may often be continued with excellent effect until the sore has nearly or quite healed. Lastly, whenever the bath, limited to the daytime, fails, continuous immersion, as advised by Mr. Hutchinson, should always be resorted to.

Some of the advantages of immersion are: its effect in nearly always quickly relieving, and often in removing entirely, the severe pain attending phagedæna. Caustics are very rarely required. The ulcer is kept clean and free from discharge

without pain or trouble. When a wound has to be made by the surgeon, its edges hardly ever become inoculated. The pain caused by frequent change of dressing is altogether avoided. The materials necessary for immersion being only those usually to be found in every house, it can be equally well carried out in private as in hospital practice.

In conclusion, I think it may be said that in the majority of cases of unhealthy ulceration and sloughing, whether local or syphilitic, immersion is the best and most speedy, as it is certainly the least painful, of the modes of treatment hitherto adopted.—*Lancet*, May 24, 1879, p. 731.

AFFECTIONS OF THE EYE AND EAR.

52.—ON GLAUCOMA.

By CHARLES HIGGENS, Esq., F.R.C.S., Assistant Ophthalmic Surgeon to Guy's Hospital.

Three forms of glaucoma are met with in practice:—1. Simple. 2. Inflammatory, divided into chronic and acute. 3. Secondary glaucoma. Certain distinctive features belong to each form and variety, but one prime factor, which is the essence of the whole glaucomatous process, belongs to all, viz. *increased tension of the eyeball*.

To what is this increased tension or hardness of the eyeball due? There seems to be little doubt that increased hardness is due to distension of the tunics of the globe by increase of the intra-ocular fluids. Assuming, then, that the hardness is due to an increase of intra-ocular contents, we have to consider—1st, in which of the chambers of the eye does the greatest increase take place, and, 2nd, how is the increase brought about? The first question can be answered by simple clinical observation. We see that in all cases of inflammatory and simple glaucoma, and in a large proportion of cases of secondary glaucoma, the anterior chamber is extremely shallow, showing that the increase of intra-ocular contents must be behind the iris; and as the communication between the anterior and posterior divisions of the aqueous chamber is open through the pupil, it is not in the latter, but behind the lens, and therefore in the vitreous. Moreover, we occasionally meet with cases in which, as the glaucomatous process advances, the eye becomes myopic, showing either that the lens has been pushed forwards to a great extent or the retina backwards. Such cases are, however, quite the exception, the rule being that as glaucoma progresses the range of accommodation becomes more and more limited, and the near point recedes from the eye.

I have more than once seen a threatened attack of glaucoma determined by the application of a strong solution of atropine, which, of course, paralyses the ciliary muscle, but may influence tension in another way, which will be referred to presently.

The age at which inflammatory and simple glaucoma usually appear is that at which the lens is becoming hardened, and calls for greater efforts on the part of the ciliary muscle, in order to accommodate the eye for its near point.

Whatever may be the starting-point of increased tension it is very easy to understand how when once set up it may not only be kept up, but very rapidly increased to an extreme degree.

The arrangement of the large venous trunks as they leave the globe, though very probably exercising a regulating influence on the intra-ocular circulation so long as the eye is entirely healthy, seems to me such, that the blood, on the slightest increase of pressure, is at first squeezed out of the vessels, and is subsequently retarded in its flow.

All the vessels referred to pass obliquely through the sclerotic, so that they run for some distance along canals in its substance. Increase of tension tends to stretch the sclerotic, which, in persons of the age at which glaucoma usually makes its appearance, is extremely unyielding; consequently it becomes consolidated or pressed together, and the venous canals are narrowed or entirely obstructed.

The arteries, with the exception of the long ciliary, pass straight through the sclerotic, and so are not subjected to the same influence as the veins; and even if they were, the force of the blood current in them could easily overcome an obstruction which would cause very serious impediment to the venous flow. This impediment to the circulation causes stasis and subsequent exudation from the distended vessels, and gives rise to increase of the contents of the globe.

Taking for granted, then, that the atrophic change in the ciliary muscle is the prime cause of the increase of tension, its probable action being that it in some manner impedes the flow of blood from the interior of the eye, and that this change has commenced, the first result will be slight and continued increase of tension, which may go on for years, vision becoming slowly impaired, but so slowly that the sufferer scarcely appreciates its failure until considerable damage has been done. This is the condition of simple glaucoma. This slow increase of tension may be interrupted by attacks of inflammation, probably brought on by any cause which increases the vascularity of the eye. Thus, overwork of the eyes, too great physical exertion, excitement from any cause, in fact, anything which hurries the intraocular circulation, may give rise to

congestion with inflammatory symptoms, and augmentation of the already too great tension of the globe. The attack may pass off, but the condition of the eyeball is never quite the same as before; it is probably rather harder, and its vision has certainly deteriorated in acuteness, extent of field, or both. This is the condition of chronic inflammatory glaucoma. Again, very severe inflammatory outbreaks, accompanied by agonising pain, total extinction of vision, stony hardness of the globe, and often severe constitutional symptoms, occur. This is acute inflammatory glaucoma. The sudden onset of these attacks is not very easily explained. I do not believe that they occur in eyes previously quite healthy. In most cases we find that there have been threatenings; the patient has had peculiar visual phenomena, at times seeing coloured rings and mists or dark clouds, often inability to do near work with or without glasses, occasionally even slight inflammatory attacks. These symptoms indicate a slight increase of tension, and point to a rigid sclerotic, and very probably the occurrence of the atrophic changes in the ciliary muscle already alluded to. In such an eye it seems probable that a sudden venous stasis may occur, followed by rapid effusion and increase of tension. This, I believe, is what happens in many cases of acute glaucoma. The sudden stasis may be caused by slight increase in the quantity of blood entering the eye, making more or less complete the already existing obstruction of the venous circulation.

Again, it is possible that from severe irritation of the sympathetic nerve temporary paralysis may occur, and its regulating influence on the circulation be lost. It is a well-known fact in physiology that stimulation of the sympathetic, though it at first causes contraction of the blood-vessels, if long continued or very intense leads to vascular dilatation.

The action of atropine in determining an attack of glaucoma points to paralysis of the sympathetic. As is well known, atropine acts as a stimulant to the sympathetic, and owing to this action causes dilatation of the pupil. I am constantly in the habit of demonstrating the effect of this drug in cases of paralysis of the motor oculi. The dilatation of the pupil in these cases varies greatly, but in all it can be visibly increased by the application of strong solution of atropine. All this goes only to prove its stimulating action on the sympathetic; and in healthy eyes I have never seen it, so far as the eyes are concerned, cause anything but dilatation of the pupil and paralysis of the accommodation; though I have often, in cases of anomaly of refraction, used a solution of gr. iv to ʒj three times a day for days or even weeks. In a diseased eye, however, the case is widely different—and here the fact already mentioned with re-

gard to irritation of the sympathetic has application—the nerve is already irritated as much as it can bear, and the stimulation of a solution of atropine gives the finishing touch, and causes paralysis with its accompanying evil, dilatation of the blood-vessels.

I do not know that atropine ever causes increase of tension in healthy eyes. I should think not; but besides having seen it determine an attack of acute glaucoma, which I certainly have, I constantly meet with cases of long-continued iritis, or corneo-iritis, in which atropine after a time has exactly the opposite effect from that desired. Instead of soothing and tending to dilate the pupil it irritates and causes contraction; if discontinued for a time, and then used with caution, it will have the wished-for effect. It may be argued that the dilatation of the pupil in glaucoma is against the theory of paralysis of the sympathetic, but my remarks are intended to apply more especially to acute forms of the disease. In all cases of both simple and inflammatory glaucoma the early or premonitory symptoms point to paresis of the ciliary branches of the third nerve; thus we have marked failure of the range of accommodation.

It is quite fair to suppose that when the outbreak of acute glaucoma occurs the remaining action of the ciliary nerves is lost, that the iris becomes completely paralysed, the pupil being neither contracted nor widely stretched, but moderately dilated. This is the condition in which we find it in acute glaucoma. The widely dilated pupil is met with in simple and chronic glaucoma.

Another case of great and sudden increase of tension is intra-ocular hemorrhage, but this is probably secondary to venous dilatation.

In the case of a man with acute glaucoma of the left eye, who was under my care three years ago, and upon whom I performed iridectomy, the tension subsided and the media cleared, but vision was nil. The ophthalmoscope showed extensive hemorrhages in the deep layers of retina or choroid; the blood had evidently been extravasated some time, for the patches had a brownish colour.

It is not uncommon to meet with hemorrhage after iridectomy, when it is probably due to removal of the pressure of increased tension upon the distended veins. This was well shown in one of my early cases at the Central London Ophthalmic Hospital. The patient was an old man, blind for years from glaucoma, but the disease seemed to have been lighted up afresh, and he was in great pain. Both eyeballs were stony hard; he would not have them both excised. I commenced performing iridectomy downwards in the left eye; as soon as the aqueous

had escaped the incision began to bulge; presently the lens escaped in its capsule, followed shortly by the vitreous, and then came a gush of blood, such as one would hardly have thought could escape from blood-vessels of the size of those in the eye. The bleeding was checked by pressure. An incision was made for iridectomy in the right eye, with an exactly similar result. The patient had no more pain; both eyes eventually shrank.

The remaining form of glaucoma is characterised by a gradual or somewhat rapid increase of tension coming on in the course of some other affection of the eyeball; it is known as secondary glaucoma. We meet with secondary glaucoma in case of iritis, corneitis, corneo-iritis, cyclitis, and occasionally after injuries of the eyeball causing displacement of the lens, or rupture of its capsule and opacity and swelling of its substance. It most frequently, however, follows perforating ulcers or penetrating wounds of the cornea, followed by anterior synechia.

There is one practical point to be borne in mind with regard to secondary glaucoma: it often attacks young people in whom the sclerotic is less rigid than in persons of the age at which other forms of glaucoma appear; so that, unless the tension be considerable, or there be great pain, there is no need to be in a hurry to perform an operation. In such patients the elasticity of the sclerotic prevents permanent damage from slight increase of tension, and as the primary disease—corneitis, iritis, &c.—gets well, the increased tension subsides also.

The distinctive features of the various forms of glaucoma are to some extent implied by their names; thus, in the inflammatory forms the increase of tension is accompanied by more or less severe inflammation. In simple glaucoma the increase of tension goes on steadily without inflammatory symptoms; in secondary glaucoma the disease, which the increase of tension follows, is very probably of an inflammatory nature, but its characteristic is that the glaucomatous symptoms come on in the course of some other affection of the eye.

Simple glaucoma attacks persons past the middle of life; it is most insidious in its onset and tardy in its course, but if unrelieved leads at last to incurable blindness.

Gradual failure of the acuteness of vision, abnormally rapid increase of presbyopia necessitating frequent changes of spectacles; peculiar visual phenomena, as coloured mists, haloes round a flame, rainbows, fiery circles and stars, and occasionally dense black clouds, which for a time entirely obscure the visual field; and inability to do near work or read for any length of time with or without glasses, are the symptoms noticed by the patient. On examination we find increase of tension, a range of accommodation less than should be found in a normal eye of

the same age, narrowing of the visual field, some impairment of central vision, shallowness of the anterior chamber, dilatation of the veins emerging from the globe in the ciliary region, a rather dilated sluggish pupil, and apparently haziness of the lens, all these symptoms being marked in proportion to the duration of the disease. The ophthalmoscope shows in early cases but little change, but in later stages we find hyperæmia of the optic disc, narrowing of the retinal veins upon its surface, with dilatation of their trunks in the retina, and pulsation of the retinal arteries, spontaneous, or easily produced by pressure on the exterior of the globe. These are all indications of pressure upon the disc caused by the continuance and gradual increase of tension of the globe. Hyperæmia of the disc is a term used very vaguely, but it need not be. If we look carefully at a healthy disc we can make out that its area is divided into three zones. In the centre is a small white, nearly circular figure, surrounding the blood-vessels, but extending further on one side—generally the outer—than in other directions; next comes a pink band, which makes up the greater part of the disc, this being again succeeded by a white ring.

In hyperæmia of the disc it is this middle pink zone which requires attention. Instead of the normal pink colour it becomes deep dusky or even bluish red.

Apparent haziness of the crystalline lens must also be carefully noted. The ophthalmoscope will show that there is no real opacity, it is merely the ordinary senile change, which is more plainly seen than in a healthy eye on account of the dilatation of the pupil.

Later, cupping of the optic disc is seen, but the cup takes long to form, and it is only in cases of months' or even years' standing that we meet with a thoroughly well-developed glaucoma cup. Later still, atrophy of the optic nerve and retina, and hopeless blindness supervene.

Inflammatory glaucoma also attacks, almost exclusively, persons past the middle of life.

Chronic inflammatory glaucoma, or chronic glaucoma as it is generally called, is, as already stated, characterised by the occurrence—at longer or shorter intervals—of attacks of inflammation.

The patient has probably had for some time what are known as premonitory symptoms, *i. e.*, he has experienced some of the visual phenomena noticed in simple glaucoma, and, in addition, he will tell us that there have been one or more attacks of inflammation and pain. We shall probably be applied to during one of these attacks. We find, in addition to the increased tension, which is very marked, fulness of the veins emerging

from the sclerotic in the ciliary region, ciliary redness, increased conjunctival vascularity, steamy and partially anæsthetic cornea, general haziness of the refractive media, shallow anterior chamber, dilated fixed pupil, and vision greatly impaired, or even reduced to perception of light.

The ophthalmoscope will reveal but little, the media being so cloudy that no details of the fundus can be made out, a dull red reflection only being returned, or, at most, the optic disc being indistinctly seen.

As the inflammatory attack passes off the media clear, and vision returns, but is never as good as before; this is the period of "remission," which may last for a variable time, in some cases a few weeks, in others months, or even years elapsing before a fresh outbreak occurs. If we examine the eye during a remission we find conditions similar to those met with in simple glaucoma.

These remissions constitute one of the chief dangers of chronic glaucoma. The patient who has passed through an inflammatory attack finds his sight so much improved during the remission that he is inclined to believe himself cured, but the tension of the globe is still increased, the retina being slowly and surely destroyed by pressure, and sooner or later a fresh attack will occur.

The chief characteristic of acute glaucoma is its sudden onset and the violence of its symptoms.

The patient having passed through a premonitory stage, as a rule, of no very long duration—the symptoms being often so slight as hardly to arrest attention—is seized quite suddenly, and often during sleep, in the manner described at page 206. The inflammatory symptoms affect not only the eyeball, but the eyelids and integuments of the face, so that cases have been mistaken for erysipelas.

The patient usually presents himself within a few days of the attack. We find some swelling and redness of the eyelids, intense congestion of the globe, often swelling of the conjunctiva, the cornea steamy, anterior chamber shallow, pupil moderately dilated and fixed, media extremely hazy, globe as hard as stone, and vision reduced to perception of light or entirely destroyed; the pain has probably subsided. The ophthalmoscope reveals nothing but cloudy media.

As a rule, unless relief be given, though the more acute symptoms gradually subside, the eye is left permanently hard and completely blind. Occasionally, however, the inflammatory symptoms pass off, the tension diminishes, and vision returns, but only to be destroyed by a subsequent attack.

The treatment of glaucoma is almost entirely operative, but in the early stages of simple, and the premonitory stage of

chronic and acute glaucoma, we may with advantage employ palliative means. Thus, patients should not read or do near work either with or without glasses, should avoid bright light, be careful in diet, and take little or no stimulant, keep the bowels pretty active, and avoid great exertion, bodily fatigue, and mental worry. The less we do in the way of applications to the eye the better; atropine and belladonna must not be employed.

In simple glaucoma, as soon as we are satisfied that the disease is advancing, however slowly, and in the chronic and acute forms, so soon as the globe has become decidedly hard, or an inflammatory attack has occurred, an operation should be performed without delay.

Many patients suffering from glaucoma will allow one eye to go quite or nearly blind, or vision of both to become much impaired before seeking advice.

In all cases before performing an operation the time the disease has existed and the state of vision must be carefully considered; it is not enough to know that the eye is hard and vision impaired or lost; these, especially the former, are the indications for immediate operative interference; but we must remember that an eye, which is hard and blind, may be hopelessly blind, the retina having been subjected to pressure too long to recover its function, even though the tension be effectually reduced.

In simple glaucoma the pressure is so gradual that the retina becomes almost imperceptibly destroyed, but so soon as any part has become blind it is lost and will not recover itself, so that all that can be hoped for from an operation is that the eye may retain the vision it has at the time of its performance; no improvement must be looked for; consequently an operation should not be thought of in cases of simple glaucoma in which vision has been reduced to perception of light.

In chronic glaucoma some improvement often follows the reduction of tension, but even here we must be careful not to promise too much. The improvement is most marked if the patient be operated on during, or shortly after, an inflammatory attack. In such cases vision, which may be reduced to perception of light, after reduction of the tension, becomes rather better than before the inflammatory attack occurred. If, however, vision during the "remissions" is quite or nearly destroyed, he must expect little or no improvement.

In chronic, as in simple glaucoma, reduction of tension stays further destruction of the retina, and may also cause some improvement of vision. The gain is, however, not very great.

It is in acute glaucoma that we see the most marked result from an operation; vision may be reduced to perception of

light only, and may have been in this condition for days, or even weeks, and yet very good vision follow reduction of tension.

Secondary glaucoma is chronic in its course, and the same remarks apply to it as to chronic glaucoma.

The operation generally performed for the relief of increased tension of the eyeball is iridectomy. Sclerotomy and puncture of the ciliary body have each their advocates. My experience of the former is altogether unfavourable, of the latter I know nothing.—*Guy's Hospital Reports*, 1879, p. 25.

53.—INJURIOUS EFFECTS OF TEA-DRINKING HABITS UPON THE NUTRITION OF THE EYEBALL.

By Dr. J. R. WOLFE, Glasgow.

The affection of which Dr. Wolfe spoke was softening of the vitreous humour, which became filled with floating particles of pigment. It had come under his notice in persons who at first sight seemed to have very little in common. He had found it among—1. The mining population, who pass a deal of time underground; 2. Washerwomen; 3. Middle-aged labourers, masons, and out-door workers; 4. Shop and factory girls; and 5. Not a few belonging to the upper classes. His attention was specially directed to the affection by its frequent occurrence among Australians who came to consult him. He could discover no assignable cause for the disease, either in the tissues themselves, or in the history of the patient; and it was only in directing his inquiries as to their diet, and finding that they all agreed in consuming large quantities of tea, that he came to suspect its agency. A comparison of the numerous cases of opacity of the vitreous humour occurring among tea-drinking populations, with its less frequency in France, Germany, and America, and its rarity among the Turks, tended to confirm his suspicions. Physiology did not suggest an explanation; but chemistry pointed to theine and tannic acid as most likely to cause disease. Theine might be left out of consideration, being identical with caffeine, which was innocuous; so there only remained tannic acid. This precipitated albuminoids from their solutions; hence it probably acted injuriously by precipitating some of the most important constituents of the food, and also by affecting the mucous membrane of the stomach and alimentary canal, and thus preventing digestion and assimilation. Some observations had been made as to the effects of tea-drinking on the healing of wounds and ulcers by a Glasgow surgeon, who had noticed that, in persons addicted to this habit, they took on a sort of scorbutic character. Physicians also ascribed

numerous cases of rebellious dyspepsia to the use of tea. The disease of the vitreous humour above alluded to could hardly be an isolated pathological fact, but must be associated with deleterious changes in other parts of the economy, and probably only made its appearance in organs which had a predisposition to be so affected. Without venturing upon any theory as to the action of tea on the vitreous humour, he would point out that the first expression of acute irritation of the fifth nerve in sympathetic ophthalmia was opacity of the vitreous humour and detachment of pigment from the whole uveal tract. So it was possible that chronic irritation of the same nerve might give rise to such changes in the nutrition of the eyeball as to bring about the condition under consideration.—*British Medical Journal*, August 30, 1879, p. 328.

54.—ON A NEW ACOUSTIC APPARATUS; AND ON INTENSIFIED SOUND AS A CURATIVE AGENT.

By Dr. WILLIAM A. McKEOWN, Surgeon to the Ulster Eye, Ear, and Throat Hospital, Belfast.

I have pleasure in bringing to the notice of the profession an acoustic apparatus, constructed with a view to aid the deaf in the most convenient and useful manner. Whilst this is the immediate object of the invention, I shall endeavour to show that its long continued use will exercise a curative influence over certain rebellious diseases of the ear, hitherto practically beyond the pale of medicine and surgery.

As the latter consideration is of even greater importance than the former, I will first deal with the grounds for my belief in the curative influence mentioned.

Toynbee's Pathological Researches show that Vibratory Motion ought to be a rational remedy for many cases of Deafness. To Toynbee the world is probably more indebted than to any other man, living or dead, for having put aural diseases on a scientific basis. From his tables, giving the results of his dissection of upwards of 1,000 diseased ears, it appears that a large proportion showed lesions in the 'conducting apparatus of the middle ear. I extract the following figures:

126 cases—membrani tympani thicker than natural.

59 cases—adhesions of membrana tympani to different parts.

202 cases—membranous bands between ossicles.

99 cases—ankylosis of stapes to fenestra ovalis.

66 cases—too rigid attachment of stapes to fenestra.

I am aware that doubts have been suggested by subsequent writers as to the pathological character of some of the bands described, and, also, that Hinton thinks that Toynbee's proportion of rigid and ankylosed joints is excessive, because the

most of the subjects were old and more exposed than people in general to influences likely to disorganise the auditory apparatus. Still, that the condition of rigidity is one of most frequent occurrence in practice does not admit a doubt; indeed, it is highly probable that a large proportion of the cases described in books as "Chronic Inflammation of the Drum," and "Dry Catarrh of the Drum," are cases of simple rigidity of some of the parts induced by the advance of age or constitutional causes. These terms are often only a cloak for defective knowledge, and serve the same purpose as the term "Nervous Disease of the Ear" used to do. Besides, in cases in which there has been undoubtedly a chronic inflammation, this generally brings about a lasting deafness by the induction of rigidity. In any event it will be wiser, instead of trying to explain away Toynbee's results, to accept them as a fair index of the pathological condition in many cases of chronic deafness in old people, and as a guide to rational treatment. We have, I submit, clear grounds for dealing with stiff joints in the ear as we would with a stiff knee or ankle-joint, viz., by appropriate motion.

The structure of the Ear points to Vibratory Motion as Curative.—The structure of the ear, constituted as it is of vibrating membranes, of ossicles with joints, of fluid, and of vibrating parts in connection with the nervous elements, points to it as a mechanical organ, and as one likely to require treatment on mechanical principles. For a stiff joint we prescribe exercise, for a rusty hinge we employ motion. If the muscles and joints of a pianist become impaired in their delicacy of movement, we prescribe much practice. By use, the hinge comes to work sweetly, and the muscles and joints often re-acquire their lost powers; so it is reasonable to infer that the stiff membranes, joints, and other structures of the ear, being brought into exercise by vibratory motions sufficient to set them in play, may advance more and more to a condition of health.

Recorded Experience supporting these views.—The paucity of observations on this subject is somewhat remarkable. We find only scattered fragmentary remarks in different authors of the advantage of exercise of the ear, and a few instances of the influence of strong vibrations, but no author seems to have been impressed with the important lesson taught. Willis (1680) reports the case of a woman who could hear the voice only when a tambour was beaten near to her, and Fielitz notes the case of a shoemaker's son who heard conversation only whilst leather was beaten on a stone by a hammer; Toynbee mentions a case of sudden restoration of hearing by a loud shout, but the improvement only lasted for a few days. In Hinton's book on the Questions of Aural Surgery, another case of sudden restoration is recorded. We find in several authors casual

remarks of the curative influence exercised by the sounds of the human voice intensified by tubes.

Inference from and explanation of the symptom "Hearing better in a Noise."—I will pass now from these rare and extraordinary cases to the consideration of a symptom very frequently stated by patients, viz., hearing better in a noise. The woman mentioned by Willis, and the shoemaker's son, heard better in a noise, but very many have a similar experience; they may mention the fact over and over again to successive professional advisers, but their statement is either regarded with incredulity, or as true but beyond comprehension, or is explained on some utterly insufficient hypothesis. One thing is laid down clearly enough, viz., that the symptom is of bad omen. I trust I may not be thought to value too lightly the views of many distinguished aural surgeons, when I state that not only is "hearing better in a noise" an absolute truth, but it is perfectly capable of explanation, and is a most important guide to treatment in many cases. If the hearing be better in a noise, why not let the patient have more noise? The deaf can usually bear a good deal without inconvenience. What would stun a healthy ear only brings many deaf persons to the hearing point.

I have at present a young gentleman under my care, who is so deaf that, whilst he is in a quiet room, I must speak distinctly and close to the ear to cause him to hear, but, whilst walking in the street or riding in a railway train, he can converse with me with facility. I recently asked a lady whether she heard better in a noise; she replied that she heard quite well in a noise. Her daughter, who accompanied her, and whose hearing was acute, assured me that her mother heard much better in a noisy street or in a railway train than she did herself; in fact, it was only in quietness that her mother was deaf. Do the laws of physics not explain this apparent anomaly? Is the improvement not owing to the fact that parts whose rigidity has been overcome to some extent by strong vibrations, more readily respond to the minor impulses of the human voice? It is well known that vibrations of varied degrees of intensity and pitch may all at the same time traverse the same media, each system preserving its individuality. We know that it is difficult to set a laden wagon in motion, but once the inertia has been overcome, small forces may afterwards materially contribute to its advance. So with the rigid structures of a diseased ear.

Remarkable improvement in cases of profound Deafness by intense Vibrations.—Whilst this paper was in course of preparation, it occurred to me to imitate the plan adopted by the woman two hundred years ago. I selected cases of very long standing in which the hearing was so bad that words uttered close to the ear were only heard as sound, or in which it was necessary to

speaking in a clear voice close to the ear to make the patient hear. As the cases are at present under observation, I will reserve details for a future paper. I may state here, however, that the results have been surprising, and quite beyond my most sanguine expectation. I adopted the simple plan of causing the patient to sit in the acoustic chair, to be afterwards described, and whilst so seated, with the nozzles of the tubes in the ears, I caused a large roll of sheet-tin, which chanced to be at hand, to fall violently and repeatedly on the floor. I found that, in some cases in which words spoken loudly close to the ear were not distinguished, the hearing became so far restored after this treatment that the words could be distinctly heard. In other cases, the voice heard before treatment only close to the ear, became audible afterwards at the distance of two or three feet. Recently, a man, 71 years old, very deaf for many years, able, when he entered my consulting rooms, to hear with each ear a loud voice close to the ear, came to hear, after a sitting of a few minutes, every word I said at the distance of two feet from one ear, and four feet from the other, without the aid of tubes. To-day, similar treatment was repeated with the result of improving the hearing distance of one ear to six feet and of the other to eight feet.

I can draw only one inference from my own limited experience of the influence of intense vibrations, viz., that very many cases hitherto regarded as hopeless will be restored to useful hearing by the simple method described.

Inference from the Continuous Exercise of the function of the Healthy Ear.—But whilst we may reason from the pathological conditions, the structure of the ear, recorded experience, and personal observation, we may take a lesson from the work daily performed by the healthy organ. Few have thought of the incessant exercise of the ear when in a state of health, or of the great evil of prolonged disuse in the case of the deaf. A normal ear during the whole of the waking hours is in a state of continual vibration; the human voice, the noise of the streets, the whistling of the winds, the rustling of the trees, and the innumerable noises pervading all nature, keep the ear in continual exercise. What a contrast for the persons who are deaf to a considerable degree! Excepting loud noises or the voice of a speaker close to them they hear nothing—the ordinary aerial vibrations are not strong enough to affect the delicate vibrating apparatus of the ear, prolonged rest is the result, and this rest makes the organ still less capable of responding in the future. A healthy ear has sixteen or eighteen hours of daily use, an impaired one has often not an hour. Should it not be our object to render it possible for deaf persons to use their organs for hours daily?

The ear is quite analogous to other parts of the body. The most perfect development of bones, muscles, and joints, can only be brought about by much work. Weak and atrophied muscles and stiff joints are the invariable result of want of exercise. To have a perfect organ we must have exercise and plenty of it. If this be necessary to maintain the functions of a healthy organ, how much more is it so to restore an impaired one!

Objections to the Ear-Trumpets and Tubes hitherto in use.—Now let us inquire whether any of the instruments for intensifying sounds hitherto in use are adapted for prolonged application, or are generally of sufficient magnifying power to enable deaf persons to hear distant sounds, or are so arranged as to be applied to both ears at the same time when desired. An acoustic apparatus which fails in the first two particulars cannot admit of that prolonged use which is essential, not only for the pleasure of the individual, but for the necessary exercise of the function of the ear.

The acoustic cornets with a spring over the top of the head are mere toys, and are only useful in the slightest degrees of deafness. The stiff tubes of every variety of form to be held in the ear, are generally far too weak to enable persons deaf in a considerable degree to hear conversation, even at a short distance, and cause so much fatigue of the arm, that deaf persons would rather bear their infirmity than use them for a lengthened period: certainly I have never met a person who used such a tube to each ear. The large sound-collectors placed on stands, with long flexible tubes leading to the ear, are not well adapted for distant vibrations, and are also inconvenient. The long narrow tube impairs the sound very greatly. As to the long flexible tubes, having a nozzle at one end and a small receiving bell at the other, to which the speaker is to address himself, they are only of use for very short conversations and for very deaf persons.

The uselessness in general of all acoustic apparatus hitherto in use will be at once apparent to anyone who knows the history of deaf persons: they are practically debarred from social intercourse, their auditory organs lie fallow, and their mental powers become very much blunted from want of training in the great school of the world. They feel they are comparatively useless members of the community, and anything recommended as calculated to relieve them secures a patient trial. There is not an aurist or instrument-maker within reach who does not receive a share of their patronage. They are provided with a regular armamentarium of cornets, trumpets, and tubes, which they only use when absolutely obliged. If possessed of sufficient means, they go from watering-place to watering-place,

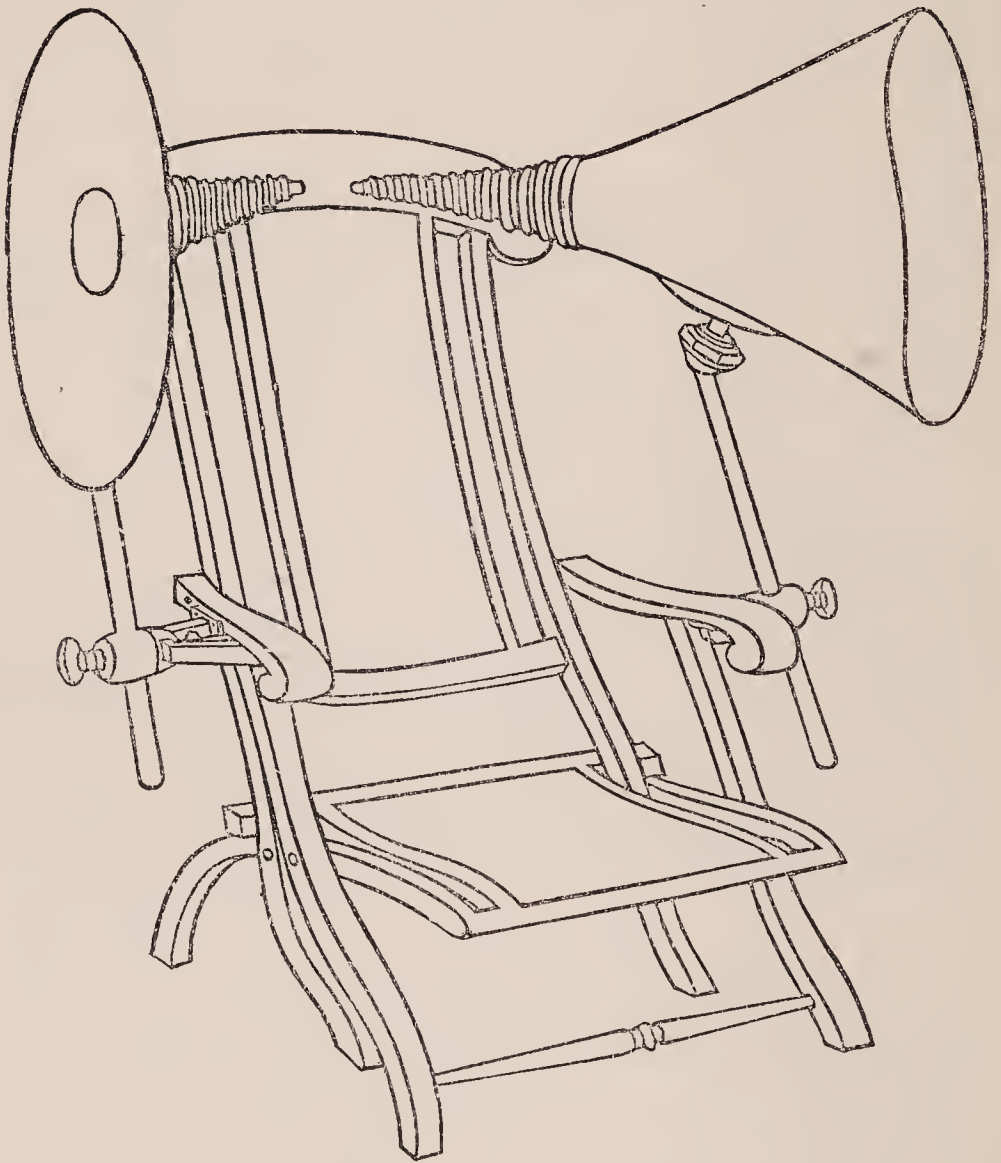
and climates hot and cold are tried in succession. I have even heard of persons ascending the Alps, in the hope that the greater rarity of the atmosphere in these elevated regions would give relief. After trying everything, however, they usually come to regard their malady as incurable, and become the resigned victims of a gradually increasing deafness.

The Magnifying Power necessary.—It is little wonder that the tubes in common use have little effect in improving the hearing power for distance, when we bear in mind the law of acoustics, that the intensity of sound diminishes with the square of the distance. Suppose a person hear the voice at the distance of two feet from the ear, what is the magnifying power necessary to make the same voice heard at the distance of ten feet? According to the law stated, the tubes would require to magnify twenty-five times.

How I propose to obviate all the defects stated.—The idea occurred to me that I could accomplish this by making the furniture of home and office acoustic apparatus. Nine-tenths of a man's time is spent at home or business; and if during this period I could secure the easy exercise of the function of hearing, the advantage resulting to the deaf would be incalculable.

By this plan it is clear that very large tubes may be used; but it was essential not only to use the large tubes, but have them so arranged that the terminable nozzles should remain in the ears, when introduced, without requiring to be held in place, that they should not press on the ears injuriously, and that the head might be moved without hurting the ears. I will not weary the reader by the details of my numerous experiments, but will state that I finally succeeded in fulfilling these requirements in the following way. I attached to each side of a chair, as illustrated in the accompanying drawing, a large tube consisting of a rigid and flexible part in combination. The drawing sufficiently illustrates the mechanism by which the tubes may be raised and lowered, shifted backwards and forwards, and revolved so as to admit of the most perfect adjustment. The flexible termination is, perhaps, the most important part of the apparatus, and requires some explanation. When conducting my experiments, I had very great trouble in finding a suitable flexible part. The India-rubber tubes met with in commerce had always some serious fault; they were either so rigid as to hurt, so flexible as not to keep their position, so small as to diminish the volume of sound, or they had obstructions inside to the passage of sound, or were liable to closure of the canal on being bent. I required a tube of sufficient rigidity to maintain its shape and position, so that it would not require the aid of the hand to keep it in place, of sufficient flexibility to permit of its being moved freely in every direction on very

slight pressure, without materially diminishing the bore of the tube, and of sufficient diameter to admit of a large volume of sound. It should have a smooth internal surface so as to have as little irregular reflection of sound as possible. It should admit of being shortened on slight pressure on the nozzle, so that if the head were moved against it the tube would shorten, and the ear be thus saved. I was much puzzled for some time as to how I should obtain a tube having all these properties, some of which almost seemed to be antagonistic to the others. At last I took a lesson from nature. It occurred to me that



the trachea possessed all the properties specified, and I therefore made a tube, taking it for a model. On a shape of the desired form, a thin layer of India-rubber was moulded, and on this a spiral of the same material was coiled externally; rings would also suit. These tubes may be made of many varieties, curved,

straight, cylindrical or conical. The woodcut illustrates the form I think best. On trial, I found that the tubes fulfilled all my requirements and rendered a binaural apparatus capable of easy use.

It will be observed that the mouth of the tube figured in the drawing is oval. It may, however, be spherical, or its end may be truncated, or it may be bell-shaped. No doubt different forms will be selected by persons with different degrees and varieties of deafness.

Advantage of Binaural Hearing.—Few persons appreciate fully the value of two ears. Let a person who hears well close one ear whilst riding in a carriage or railway train, and he will observe a remarkable deadening of the sound; so when a person testing the tubes closes one ear, he will find that the increase of sound is not at all to be compared to the increase when both ears are used. I believe the intensity by binaural hearing is much more than doubled.

The Proper Power to be used.—The fundamental rule should be in all instances to use tubes of sufficient power to cause the patient to hear easily, if possible; and subsequently, if improvement take place it may be desirable to diminish the power.

Prejudice against Tubes Irrational.—It is well known that many entertain an unreasonable prejudice against the use of tubes. What harm can there be, in case the conducting apparatus of the ear do not convey ordinary vibrations, to so increase the intensity of the sound as to enable the deaf to hear all the sounds to which the healthy ear is accustomed all day long? This prejudice is so totally at variance with reason and common sense, that I will not further refer to it.

Hearing-Tubes and Spectacles not entirely analogous.—This may seem a strange assertion, but is nevertheless true. Spectacles very rarely have any curative influence over defects of refraction. When spectacles are laid aside, the eyes are in the same condition as before they were put on. Tubes for the ear, on the contrary, by causing vibratory motion, render such motion on weaker impulses afterwards more easy, and are therefore curative. In the case of the eye, we have an irremediable defect of structure; in the case of the ear, a morbid condition, the stiffness of parts more or less remediable. It is an interesting question, which will probably ere long find an answer, whether the tendency to chronicity, so strongly marked in ear-affections, is not directly traceable in many instances to want of regular exercise of the function by intensified sounds.

Exercise of the Auditory Function will lead to the Cultivation of the Intellect.—Irrespectively of the mere pleasure of hearing and the prospect of improvement by exercise, efficient acoustic apparatus must be strongly advocated on this most important

ground. The majority of the deaf suffer very much from impairment of the mental faculties. In the case of a person whose mind was untutored previous to the deafness, his conversation is confined to the immediate necessities of life. He may hear questions as to his home, his family, his church, or his business; but when we pass beyond these topics, though we may speak with the same force, a laborious mental process seems to be brought into operation before he can understand. There seems an appreciable interval between hearing and understanding. In most instances, the attention must be so put on the stretch, that the deaf person prefers to avoid conversation unless compelled by necessity. The training of a deaf person must really be pursued stage by stage, like the education of a child. We must bear in mind, in dealing with the deaf, that a practised ear and a cultivated mind mutually assist each other.

Cases in which Acoustic Apparatus should and should not be used.—It should not be used

1. In nervous deafness generally (fortunately a rare disease):
2. In acute inflammatory attacks.

It should be used

1. In those numerous cases of deafness beginning without inflammation; no doubt owing, in most instances, to rigidity of parts resulting from advance in years or from constitutional peculiarities:

2. In those cases resulting from inflammation, but in which the inflammatory process has induced and left behind it changes in the conducting media which have resisted medical and surgical remedies.

Summary of Advantages of my Acoustic Apparatus.—1. By it large tubes may be used with the greatest ease and convenience, and for indefinite periods.

2. Both ears may be used at the same time.

3. Deaf persons, whilst the apparatus is in use, are put more nearly on a par with hearing persons as regards the exercise of the function.

4. Deaf persons are enabled to attend to business from which they would be otherwise precluded; for example, a judge on the bench, a lawyer in his chambers, a merchant in his office, may perform their respective duties by aid of an acoustic chair or acoustic writing-table.

5. The intellect is cultivated *pari passu* with the hearing power.

6. Improvement in the hearing is effected by the prolonged exercise of the vibrating and conveying structures of the ear by intensified sound; and there is reasonable ground for hope that persons hitherto regarded as afflicted with an incurable malady may be even so far restored as to dispense for the most part with the use of tubes.—*Brit. Med. Journ.*, July 5, 1879, p. 4.

55.—PROFESSOR HUGHES'S AUDIOMETER.

In a recent paper read before the Royal Society by Dr. B. W. Richardson, he described a wonderful new instrument devised by Professor Hughes. This instrument is associated with a microphone and a telephone, indeed it would have been impossible without them. It has been christened the audiometer, and will no doubt become an indispensable help both to the aurist and general practitioner. The fact that an electric current traversing a coil of wire will induce another current in a separate coil placed near it has been constantly brought home to us of late months through accounts of the various electric light machines. It is this principle of induction upon which Prof. Hughes depends for the beautiful instrument which he has just given to the world. By placing the telephone at the ear of a deaf person, it is possible, by moving the key and adjusting the movable coil upon the scale, to test the amount of hearing which he possesses, that is, of course, supposing him to be capable of defining a sound at all. The importance of this instrument both in registering the progress of a case, or for testing the powers of candidates for appointments where good hearing is a *sine quâ non*, cannot be over-estimated. Dr. Richardson has already found it most useful in his practice, and more especially in some cases where artificial tympanums have been resorted to. In one case, where a patient's ear could only detect a sound up to 110° on the scale of the audiometer, the artificial drum gave him an immediate gain down to 60°. It was long ago prophesied that the microphone would be of great use to mankind, and we are glad to find that one of its first applications is towards the alleviation of the terrible malady of deafness. Both the medical profession and the public generally must therefore heartily thank Prof. Hughes for the audiometer. —*Edinburgh Medical Journal*, July 1879, p. 79.

DISEASES OF THE SKIN.

56.—THE TREATMENT OF ECZEMA OF THE HAND, OFTEN MISCALLED PSORIASIS PALMARIS.

By Dr. JOHN KENT SPENDER, Bath.

How I wish that our masters and teachers of dermatology would make it plain beyond all cavil that there is no such thing as *psoriasis palmaris*, except as a syphilide. Dr. Liveing approaches the nearest to this decision of utterance when he writes (*Handbook on the Diagnosis of Skin-Diseases*, p. 123) that “in syphilitic psoriasis....the palms and soles are often affected, but never in simple psoriasis.” But, if the student or

junior practitioner turn to the lamented Dr. Tilbury Fox's splendid Atlas of Skin-Diseases, he will find a plate adopted from Willan and Bateman, and described as *psoriasis palmaris*, a "very obstinate form of psoriasis." It is true that Dr. Fox admits "a scaly thickened condition, with more or less fissuring of the palms of the hands and soles of the feet," as liable to follow eczema and some other affections; but he says that it may be also a part of general psoriasis, which has travelled on to the palm of the hand from neighbouring portions of skin.

Acknowledging that the circumferential tracts of the palm may be affected with true psoriasis which has extended from the back of the hand, I confidently affirm (with the greatest respect for Dr. Tilbury Fox's learning and experience) that the disease represented in Willan and Bateman's plate is not psoriasis at all. The anatomical and physiological affinities of the skin of the palm forbid such an idea. The thing may look like psoriasis, but that is quite a different thing, as Dr. Fox would have been the first to admit. Fissures and moist scales on the flexor aspect of a limb proclaim unmistakably that a disease belongs to the eczematous group.

Dr. Fairlie Clarke (Practitioner, August 1874) has observed the confusion which arises from applying the term psoriasis to many different morbid conditions of the tongue. Dr. McCall Anderson describes the eczema of the hand of which I am now speaking as *eczema rimosum*.

The practical point is this. Wrong names affixed to diseases of the skin suggest and invite wrong treatment. Eczema of the palm of the hand is so disguised and altered by the thickness of the dermal structures, that it is hard to believe the heaped-up, fissured, and often bleeding epidermis to be an eczematous affection. But it is certain that any application of an irritating nature will exasperate the disease, and anything of a specifically soothing nature will gradually cure it.

A gentleman a little past middle age, of a healthy constitution, and engaged in the Civil Service, came to Bath from London last autumn with an eczema of the hands and feet more intense than (I think) I had ever seen before. He had been under the care of a distinguished London surgeon, who had called it eczema, but had certainly treated it as a psoriasis; for *liquor carbonis detergens* had been prescribed in a lotion, and arsenic had been given internally. The result was most disastrous. No treatment with thermal waters could permanently benefit such useless hands and painful feet. The hands were alternately bathed with glycerine and covered completely with compound lead ointment (the form I always use is given in Mr. W. Spencer Watson's book on Diseases of the Nose and its Accessory Cavities); and light thread gloves were constantly

worn. Only the ointment was applied to the feet. At a later stage, the parts were washed with milk and sulphur soap; and towards the end of the treatment arsenious acid was ordered in the form of pills. At the expiration of about three weeks, my patient left Bath much better; he got through the terrible winter without serious drawback, and in February he was virtually well. For some time, he used the ointment occasionally; the smoothness and flexibility of the palms of the hands are perfectly restored, and he can walk any reasonable distance with ease and comfort.—*Brit. Med. Jour.*, June 21, 1879, p. 932.

57.—OLEATE OF ZINC IN THE TREATMENT OF ECZEMA.

By Dr. JAMES SAWYER, Physician to the Queen's Hospital, Birmingham.

After using the remedy for nearly six months, in a large number of cases arising in hospital and private practice, I desire to record my testimony in favour of the efficacy of the ointment of oleate of zinc in the treatment of eczema. For this important addition to the materia medica, the profession is indebted to Dr. Radcliffe Crocker of University College Hospital, who gave the formula for its preparation, and some cases illustrative of its use (*Retrospect*, vol. 79, p. 299). The writer concluded his observations with the following words: "I have treated a large number of cases with this remedy with most satisfactory results, so that I can with confidence recommend it as one of the most useful preparations for eczema that we possess." This statement is very unequivocal; I can as unequivocally adduce my own experience in its confirmation. I have always used the oleate of zinc made into an ointment, either with vaseline or with lard. The preparation with vaseline I have employed in private practice, and that with lard, on account of its comparative cheapness, for my hospital patients. Vaseline is preferable to lard, because it is not so liable to change, but also because the greasiness of the latter injures a patient's clothes and sometimes disagrees with his skin. The ointment made with vaseline may be rendered more "elegant" by the addition of one drop of otto of roses to each ounce. My house-physician, Dr. John Wood, informs me that he has found the oleate of zinc ointment very serviceable in the treatment of eczema, and especially in the eczema capitis of children. One of my patients, a sexagenarian clergyman, the subject of eczema, probably gouty, of several years' standing, affecting the lower part of the abdomen, the genitals, and the upper portions of the thighs, tells me that he recovered completely after using the vaseline ointment for a few weeks.—*Brit. Med. Jour.*, April 19, 1879, p. 586.

58.—REMARKS ON THE TREATMENT OF CHRONIC ULCERS BY MARTIN'S BANDAGE.

By A. F. MCGILL, Esq., F.R.C.S., Surgeon to the Leeds Public Dispensary.

The treatment of chronic ulcers of the leg in out-patient hospital practice has not hitherto in my experience been attended with great success. It has often consisted in the prescribing of some lotion or ointment, and in the administration of good advice which the patient has been unable to follow. It is an easy thing to tell a patient to go to bed for a month or two, but unfortunately a man who has to work for his family, or a woman who must support herself, often finds that rest is the last thing possible; if they wish to be admitted into a hospital, they are told that "ulcerated legs of long standing" are according to law not admissible, and so they go on month after month, and year after year, with an unhealed leg which is a source of discomfort to themselves and of annoyance to their friends.

Since the commencement of the present year I have used with excellent results the solid rubber bandages introduced by Dr. Martin of New York. The bandage has been put on next the skin without any other dressing: the patient has been instructed (1) to remove the bandage on going to bed, (2) to wash the leg with warm soap and water, (3) to sponge the ulcer with carbolic acid lotion and keep this applied all night, (4) to have the bandage well washed and hung up unfolded during the night, (5) to again wash the leg in the morning, and (6) to apply the bandage before leaving bed.

The following cases are fairly typical.

1. T. N., æt. 60; small ulcer, size of half-a-crown which had been unhealed for six months. The skin over the whole leg is red and shiny, he has been suffering on and off for twenty years; cured in a fortnight, though similar ulcers had hitherto always taken six months to heal: when offered the price of the bandage in return for it said he "would not take a pound for it."

2. H. R., æt. 56; leg ulcerated for two years, the skin is hard and callous and ulcerated in all directions, there being numerous ulcers varying from one to three inches in diameter: has not been able to walk for six months. The application of the bandage gave immediate relief and enabled him to walk. Cure in 10 weeks.

3. E. R., æt. 52; ulcer of leg 8 years, callous edges which are somewhat inflamed, measure three by two inches. The bandage caused pain for the first week, but since then has been

quite comfortable; after two months' treatment the ulcer is nearly healed.

It is unnecessary to multiply cases; of about twenty cases treated in this manner two only have been unrelieved. In both cases the want of success was due to the fact that the ulcers were inflamed and painful when the bandage was applied; the application much increased the pain and necessitated the discontinuance of the treatment. If applied in suitable *chronic* cases the bandage is more efficacious than any other method of treatment. It is, I think, a most valuable addition to the appliances used in minor surgery.—*Practitioner*, May 1879, p. 358.

59.—THE TREATMENT OF INDOLENT ULCERS BY MEANS OF SHEET LEAD.

By Dr. F. P. ATKINSON, Kingston-on-Thames.

A good deal of attention has been attracted during the past year to the American treatment of indolent ulcers by means of Dr. Martin's India-rubber bandages, and the reports received on all sides as to the value of this method are eminently satisfactory. I would, however, urgently request that a trial be given to the system which I was in the habit of adopting in all such cases at St. Bartholomew's Hospital, Chatham, some thirteen years ago, viz., the application of sheet lead, moulded to the shape of the leg and kept on by an ordinary calico bandage. The size of the lead should be sufficient to cover the ulcer completely and lap a little over the whole skin; the edges and angles should be well rounded, so as not to chafe or irritate; it should be about an eighth of an inch in thickness, and moulded very accurately to the shape of the leg, so as to allow of no indent being apparent on the surface. After it has been carefully fitted, the leg should be bandaged from the toes upwards, and all that then need to be done is to uncover the ulcer night and morning and allow some water from a sponge to trickle over it. The granulations should never be touched with the sponge itself. I believe that the rationale of this treatment is pressure, the same as in the case of the elastic bandage, though there may be also some action produced by the secretions upon the lead, as is said to take place when the lead nipple-shields are used. The great advantages of the system proposed are simplicity and cheapness, though as regards the former I think it must yield the palm to the elastic bandage. It would appear that in some parts of Africa the natives use sheet copper, and with some success, but I cannot say I have ever tried it myself.—*Practitioner*, May 1879, p. 360.

60.—TREATMENT OF ULCERS OF THE LEG BY PURE RUBBER BANDAGES.

By Dr. H. A. MARTIN, Boston, U.S.

My attention has been called to a communication from Mr. J. Fletcher Horne, appearing in a medical periodical dated April 5th, in regard to results of the treatment of twelve cases, "chosen to include varieties of the more common forms of ulcer of the leg," by the use of bandages of "pure rubber," as practised by myself for nearly twenty-five years, with *invariable* and *prompt* success, and presented to the notice of the English profession in a lengthy paper in the issue of the same journal for October 26th, 1878. Mr. Horne kept his patients in bed, as, in his opinion, "the best position for improvement." I give his own statement of results:—"After the application of the bandage each case soon presents a wonderful change. The indurated edges and unhealthy granulations rapidly break down; the gnawing pain, often complained of, is much relieved; the presence of the bandage makes the skin pink and shiny, and filled with a rash like scarlatina; healthy granulations spring up, and the wound in a short time is completely full. But here, in eight cases, followed disappointment—they did not heal. In the four successful cases, the earliest cured wore the bandage forty-two days." Mr. Horne states that "the treatment by solid elastic bandages is not a novelty, for its principle of action—uniform pressure—is old." And he concludes by saying that he "shall be glad if this elicits the opinion of some reader as to the cause of the failure to produce healing in 75 per cent. of my cases."

Without extending unduly this article, I will, as briefly as may be, say that during the employment of bandages of pure rubber alone for the very long period above-mentioned, in an aggregate of over 700 cases of ulcer of every variety and degree, I have never observed such an arrest of the reparative processes as Mr. Horne, as I doubt not truly, narrates. Before I ascertained that the bandage of "pure rubber" alone, without any other application whatever, sufficed for the best possible treatment of ulcers of the leg, I, as stated in most if not all my writings on the subject, employed various salves and lotions in connexion with the bandage. No such application is now recommended by me as conducive to the perfection or rapidity of cure, and some of these applications, one particularly—viz., carbolic acid in various degrees of dilution—hindered, greatly hindered, the process of cicatrisation, inducing precisely the same phenomena witnessed by Mr. Horne—viz., an arrest of repair at that point where cicatrisation should commence, and, as it always does when a proper bandage *alone*

is employed, proceed to a complete abolition of the solution of continuity, with a perfection, rapidity, and solidity which I never saw even distantly approached when other methods were adopted and applied, however carefully and well.

I have examined a bandage sent me on January 7th, 1879. It emitted, on opening the package, a *very* strong odour of carbolic acid, which, when it was handled and unwound, became extremely and oppressively offensive, and on exhibiting it to two very experienced manufacturers of rubber, I was informed that carbolic acid is largely used in the English manufacture of rubber, and was, very evidently, an ingredient of the sample under inspection. I have no hesitation whatever, then, in asserting that the great disappointment experienced by Mr. Horne was due to the presence of carbolic acid in the bandages he employed, and, probably, in some degree, to the fact that naphtha was used in the dissolving process which entered into their manufacture. I make this assertion with the more confidence as I have received many letters from England descriptive of like results and disappointments, and always from the use of English-made bandages—results and disappointments which I have never known when properly made bandages alone have been employed, but precisely such as I have observed when lotions of carbolised solutions have been applied under the bandage. I have examined four other samples of English bandage, and all have emitted, in varying degrees, an odour of carbolic acid and naphtha, but neither to such a degree as noted in the first specimen referred to. That four of Mr. Horne's patients did better than the remainder was due either to their skin being less sensitive to the noxious application, or, more probably, to its being present in less amount in the bandages used in their cases. It is, however, to be observed that in even these comparatively successful cases complete cicatrisation was postponed far beyond the time required for the cure of such cases by my method.

The bandages which I use and recommend are of what is technically known in this country as "pure rubber"—i.e., the very best old "seasoned" Para India-rubber, *intimately* combined with the extreme minimum of sulphur absolutely necessary, with a proper degree of heat to accomplish the perfect "vulcanisation" or "curing" of the gum. These ingredients must not be combined by solution of the rubber in naphtha or any other solvent, but by a process in which the rubber and sulphur are *thoroughly* combined without any other solvent or substance whatever. Bandages containing a particle of any material besides these two are *not* "Martin's bandages," and I disclaim, utterly and decidedly, all responsibility for effect, or want of effect, observed in their use. In

every paper that I have written on the use of pure rubber bandage, I have most explicitly stated the composition of the bandages which I recommend, and have so long and with such extraordinary success employed. I have also stated my perfect willingness to afford information to inquiring correspondents, and expressed the hope that "manufacturers may produce the bandages at the lowest paying price, but that they will not do this at any sacrifice of labour in making or of excellence of material." My experience here in America convinced me that the desire among dealers to obtain bandages which they could sell at a very low price, and still with large pecuniary profit, led to the manufacture of an article which, however attractive *in appearance*, was not fit to be used, and which proved utterly deficient in the very important quality of durability. It was this knowledge that induced me to write the communication from which the quotation is made. Soon after it was written, I began to receive by every steamer letters from England complaining of the inferior quality of the English-made bandages—the failure to fully obtain the effects I had claimed in my first paper, and which the writers obtained after they had procured bandages which had been inspected and approved by myself. Some of these writers spoke of absolutely injurious effects induced by the use of the English-made bandages—as, for instance, the severe and complete vesication of the whole leg by a few hours' application of one of them. A few of these letters contained little pieces of the bandages complained of, which were certainly very bad indeed; and two or three of the writers strongly urged me, in justice to myself and for the protection of British practitioners, to write on the subject to one of the leading London medical journals. I was very reluctant to do this. I hoped that the worthless and injurious bandages complained of might be exceptional, and that practitioners would soon learn to avoid them. At last, however, I became convinced that *all* the rubber bandages manufactured at that time in England were objectionable; that they were so because manufacturers did not appreciate the absolute necessity that they should be made in a certain way, carefully excluding ingredients in general use in the English rubber manufacture; and because also, notwithstanding the offer I had made, they had not thought it worth while to apply to me for instruction or direction; and that it was a duty, no longer to be shirked, to make these facts known. I accordingly, in the latter part of last December, wrote a "lengthy communication" to the journal in which Mr. Horne's paper appeared, on the fact of the inferiority of the English-made bandages and the reasons for that inferiority, and informing English practitioners that, in pure self-defence, and quite contrary to my

intention at the time my first paper was published (October 26th, 1878), I had been driven into the manufacture of pure rubber bandages; and also affording information where, in London, such bandages, made for and fully inspected by myself, and, as a guarantee of this, stamped with a *fac simile* of my signature, could be obtained. This paper was *not* published, its receipt was not even acknowledged in the usual way, nor was any notice whatever taken of it for over three months after its date, over two months after its receipt by the editor. Shortly after writing this lengthy communication, I received specimens of whole bandages, an examination of which so fully and authentically confirmed all, and more than all, that I had written, that I wrote a second letter on the subject to the editor, which, however, noticing that the receipt of my first epistle was not acknowledged, I refrained from sending. On the 15th March, a short editorial note appeared in the aforesaid journal, in which a most imperfect and brief allusion was made to my paper which had been in the editor's hands for over two months, concluding with these words: "To speak quite candidly, we would say that in all the respects of which Dr. Martin speaks—smoothness, elasticity, substance, excellence of material and manufacture—they" (the English-made bandages) "are *superior* to those which are imported with his signature." I immediately wrote to the editor a full statement of what I knew about English-made bandages, accompanied by eleven samples, an examination of which could not fail to convince him that he had hastily reached and expressed convictions which were unjust and, however unwittingly, untrue, and calculated to do not only a great wrong to me and the method I had originated and commended, but also to the English profession, which would undoubtedly be led by such assurances to a confidence only to be rewarded by disappointment or very partial success, as in Mr. Horne's case. I asked the editor to calmly and carefully, as the importance of the subject demanded, examine and compare the data I furnished him, and in mere justice and common fairness, as some slight reparation to me, publish the *whole* of my communication, however "lengthy," without abridgment. As I knew that he *might* not accede to this request, however just and moderate, I added a full statement of the case, together with the letter itself, to a very large edition of that paper which was presented to the American Medical Association in June 1877, in which I first publicly announced my method of treatment by the pure rubber bandage. This pamphlet is now ready, and will leave here tomorrow, and probably reach London as soon as this communication, and, without doubt, be on sale before this paper can possibly be published or acknowledged in print. To that

pamphlet I refer for full information as to the method of treatment I recommend, and the fuller details and information touching the why and the wherefore of the wide difference of effect following the use of rubber bandages which are what they ought to be, and those which are the reverse. — *Lancet*, June 14 1879, p. 839.

MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN.

61.—ON THE USE OF THE VOLSELLA IN GYNECOLOGY.

By Dr. ALEXANDER RUSSELL SIMPSON, F.R.S.E., Professor of Midwifery, &c., in the University of Edinburgh.

A young graduate just about to enter upon practice, guided by his text-books on midwifery and the diseases of women, would not think of supplying himself with a volsella as a needful part of his equipment for that department of his work. He would be duly impressed with the need and usefulness of specula, of sounds, of tents, of tenacula, forceps, etc., but if he thought of the volsella at all, he would only think of it as a superfluity. Thomas, Barnes, Emmet, Courty, Gallard, Leblond, Schroeder, Sinéty, give no place to it among the instruments required for gynecological exploration. Yet the volsella has long been a familiar instrument in the hands of experienced gynecologists. The great French surgeons of the last generation, who occupied themselves with the diseases of women, made frequent use of it. Sir James Simpson quotes Lisfranc to the following effect:—"Let a speculum be introduced so as to embrace the cervix uteri, and thus prevent the uterus falling by its own weight, then bid the patient bear down as if at stool, and you will perceive that, as the instrument descends, the uterus follows it to the extent of an inch or so from the orifice of the vagina—an immense advantage when the surgeon wishes to bring down the uterus to near the vulva. In cases requiring operation about the cervix, all that the surgeon has to do is to lay hold of the os uteri with a hook and draw it gently down until it fairly comes within sight; this may be effected without difficulty and without much inconvenience to the patient."

Dupuytren, in describing the removal of a fibroid, speaks no less distinctly as to the descent of the uterus when traction is made on the tumour—"The substance of the tumour is caught in the bite of a volsella. Moderate traction is employed, and the patient also requested to strain down as if in labour. She does this readily, so that the tumour soon appears close to the external orifice. It is now grasped with a second pair of forceps, and gentle traction, aided by the patient's efforts, brings

it to the vulvar orifice, which it soon passes, and the cervix uteri itself is exposed."

In his "Memoir on the Uterine Sound," Sir James Simpson points out that the uterus "may be drawn down by instruments till the cervix reaches the external parts themselves, or even protrudes beyond them—a circumstance which facilitates immensely the operation of the excision of this part of the organ."

Goodell has given a brief but very suggestive account of the use of the volsella, which I quote in full. He says, "One word here on the subject of the volsella. Since it maintains its hold better than the tenaculum, it is to me one of the most precious instruments in my bag, amounting in value almost to a third hand. Apart from using it as above described in re-dressing or straightening out any kind of version or flexion of the womb, it subserves other useful purposes. By hooking down the cervix and holding it steady, it materially aids in the introduction of spongetents. For the same reason, upon the removal of the tent it renders the exploration of the uterine cavity with the finger very much easier than by the usual plan of forcing the womb down on the examining finger by suprapubic pressure, a procedure always painful, and in a fat woman very difficult of execution. By thus lowering and fixing the womb, it facilitates very materially the removal of intrauterine polypi, or the scraping away of benign or malignant growths from the cervix or the fundus. In such cases I usually apply it without the aid of the speculum, and generally seize hold of the anterior lip. In re-dressing versions, a mechanical advantage is gained by seizing hold of that lip of the cervix whose name does not correspond with that of the version. But in flexions, as one object of the traction is to stretch out the flexed side the most, that lip should be seized whose name corresponds with that of the flexion. This advice is theoretically correct: but it may not always be practicable."

Batley, in an article on the "Extirpation of the Functionally Active Ovaries," says, "The cervix is seized with a stout volsella, the uterus drawn down under the pubic arch, and the vaginal membrane and cellular tissue incised with scissors, say one and a half inch in the median line of the posterior *cul-de-sac*, beginning immediately behind the uterus."

Noeggerath, in his very original article on "The Vesico-vaginal and Vesico-rectal Touch," says, "If we attempt to explore the upper section of the uterus, it must be pulled down by means of a double hook, the points of which are turned outwards, introduced into the cervical canal, and it is perfectly safe to dislocate the uterus downwards about an inch and a

half. When this is done, with one finger in the bladder and one in the rectum, we are enabled to thoroughly explore the whole of the uterus, from the fundus down to the external orifice."

The most satisfactory account of the use of the volsella we find in the writings of Hegar—first, in his *Operative Gynecology*, and again still more fully in his clinical lecture on *Gynecological Diagnosis*. In that lecture he begins his reference to the use of the volsella by saying, "For some years we have made use of a very sure, and at the same time innocent, method, which consists in applying a volsella to the vaginal portion to fix the uterus, to draw it somewhat downwards, and, where necessary, to make lateral movements with it."

In a communication which I read to this Society on "The Complete Evacuation of the Uterus," I showed how the use of the volsella facilitated the access of the finger to the interior of the uterus.

In all these quotations, however, with the exception of those of Goodell and Hegar, it will be observed that the mention of the volsella comes in almost incidentally, or with reference to some special exploration or operation; and as I become more impressed with the importance of the aid it furnishes us in various directions, I am anxious to press its value upon the Fellows of this Society, and to claim for it a permanent place in every text-book on gynecology, and in every gynecologist's armamentarium.

The Instrument.—Let me say at once that there need be nothing peculiar in the construction of the instrument. Volsellæ of different sizes are very useful, and sometimes it is absolutely necessary to have them large and long for the seizure and down-dragging of big intrauterine fibroids. In such cases I find a pair of toothed forceps very helpful, which Sir James Simpson had constructed, with separable blades, locking like a pair of Smellie's midwifery forceps, and capable of being fixed together after they were locked with a screw-pin, serving as a joint. But the instrument for daily use in the common run of cases is a simple small curved volsella of the size here represented, each stem ending in three short teeth. Sometimes single-pronged or two-pronged volsellæ with longer teeth may be employed; but I get the greatest amount of service from the small three-teethed variety, which it is better to have made with a catch on the handles like those on Pean's artery forceps.

Mode of employment.—We know the cervix uteri is richly enough supplied with sensory nervous filaments. For the most part, however, they run to the sympathetic ganglia, and but few, if any, have direct communication with the cerebro-spinal system. A prick or a crush of the lips is, therefore, even when

perceived by the individual, not a cause of acute or lengthened suffering. The cervix uteri is not only very much less sensitive than the labia pudenda, it is even far less sensitive than the walls of the vagina in the immediate vicinity. I find that when it is touched with ice the patient does not experience the sense of cold; and the touch of a cautery on the surface does not cause her pain. Hence, we feel free to use the volsella without fear of adding in any marked degree to a patient's distress.

When either the anterior or the posterior or both of the lips have been laid hold of, first the cervix, and with it the whole uterus, can be pulled forwards or backwards, to the right or to the left, but, above all, it can be dragged downwards. The amount of force required to do this on the living subject is exceedingly slight, as I find, on observation of a series of cases, that a force of from 3 to 7 lbs. is sufficient to bring the os uteri to the ostium vaginæ, without any discomfort to the woman.

I had a favourable opportunity recently of studying the mode of descent of the uterus in the grasp of a volsella in a case where the urethra had been dilated for vesical explorations, where the finger introduced into the bladder could follow down accurately the movements of the uterus and anterior wall of the vagina. As it descends, the cervix brings with it the posterior angle of what Dr. Hart describes as the pubic triangle or anterior segment of the pelvic floor; that is to say, the anterior wall of the vagina, the bladder, and the uterus are brought down *en masse*. The posterior wall of the vagina becomes inverted from above downwards more gradually, being separated from its loose contact with the anterior wall of the rectum, the canal of which remains unaffected. In general the organ must descend so far as to bring the os clear through the vulva before the utero-sacral ligaments are put upon the stretch. It is when these become quite tense that the patient has any sensation of special discomfort. We need not stop to point out that the uterus in this temporary position does not present the usual relations of the prolapsed one.

Special Uses.—I. *Exploratory.*—Let me ask attention, to begin with, to the services it will render us in the exploration of various conditions of the pelvic organs.

1. *To bring Parts within Range of Vision.*—In the great run of cases, two fingers introduced into the vaginal canal can be used to pull back the perineum, and then a finger, or sound, or pair of dressing forceps, or special depressor, pressing forward the anterior wall of the vagina, brings the cervix into view. Instead of the finger, sound, or other instrument, let the practitioner use such a volsella as I have indicated, and he will find it easy to lay hold of the anterior lip of the uterus, so as to bring os, cervix, and when necessary vaginal roof, within full range of vision.

2. *To bring Parts within Reach of Touch.*—When the uterus is thus drawn upon, it is obvious that the finger will have more easy access to the higher parts of the uterus, whether it be passed first through the cervix into the interior of the uterus, or whether it seeks to determine the condition of it and its adnexa externally, and its relations through the vaginal canal, through the rectum, or in rarer cases through the bladder, or through two or more of these simultaneously. We thus get a better impression, *a*, of the size of the uterus, whether that be normal, lessened, or increased; *b*, of its position, when it is the subject of version or flexion in one or another direction; *c*, of its relation to pelvic tumours, whether it be attached to them or independent in its mobility; *d*, of the size, situation, and circumstances of the ovaries and other parts around the uterus. I do not need to state that the use of the volsella which I am describing does not exclude the employment of other instruments for exploration, and in some cases, after laying hold of the uterus with it, or before doing so, it will be convenient to make use of the speculum, or more rarely of the sound.

II. *Operative.*—Without entering into details of the various operations, I may point out some in which it is either absolutely necessary or extremely useful to bring down the uterus, or simply to fix it with a volsella.

1. *Operations on Interior of Uterus.*—It will be found very helpful, 1st, in *Introducing* stem-pessaries, tents, medicated arrows, or the sound, armed with cotton wadding, and charged with various medicaments; 2d, In the *Removal* of foreign bodies, or of neoplasms and fragments of ova, from the cavity of the uterus, whether with the finger or by means of a curette or other instrument.

2. *For Application of Elastic or Esmarch Ring.*—To carry out the various operations on the vaginal portion of the cervix bloodlessly, an India-rubber umbrella ring may be carried up to the level of the isthmus uteri by passing it over a volsella that has grasped the whole thickness of the cervix in its embrace, or over two volsellæ, one applied to the anterior, the other to the posterior lip.

3. *Operations on the Vaginal Portion of the Cervix.*—In the many cases where recourse must be had to operative interference with the os and cervix uteri the use of the volsella is simply indispensable. Such are, 1st, Division of the cervix; 2nd, Amputation of its infra-vaginal portion; 3rd, Emmet's operation for the repair of lacerations of the cervix; 4th, Removal of polypi, etc.

4. *Operations for Rectifying Displaced Uteri.*—In restoring the uterus to its proper position in cases of flexion, version, and inversion, the volsella facilitates immensely the reposition.

5. *Operations on the Vagina.*—In attempting the closure of a fistula high up in the vaginal cavity, or the removal of neoplasms in that situation, the manipulations can in many cases only be satisfactorily carried out when the uterus has been pulled downwards and backwards with the volsella.

6. *Intra-Peritoneal Operations.*—In cases where we seek access to the peritoneal cavity through the vaginal roof, as for the removal of the ovaries, extra-uterine ova, etc., the volsella, grasping the cervix, pulls it forwards and downwards, and makes tense the structures that require to be divided.

Contra-Indications to Use.—I have said that in advocating the more frequent use of the volsella I do not wish it to be understood that it should supplant the use of the speculum, sound, and other instruments with which the profession has long been more familiar. I would now add that, as in the employment of these instruments, so in the employment of this, it must always be borne in mind, that notwithstanding the great assistance which we obtain from it, there are conditions, physiological and pathological, which forbid its application.

I. *Physiological.*—As a general rule, which has but rare exceptions, the use of the volsella must be abstained from in the two physiological conditions: 1, of *Menstruation*; 2, of *Pregnancy*. Even in cases where morbid conditions of menstruation or pregnancy call for active interference, the wise practitioner will use the volsella as he would practise the simplest touch in a menstruating or pregnant female, with more than the usual care and delicacy.

II. *Pathological.*—1. *Neoplasms* rendering the tissues of the cervix particularly friable or hemorrhagic prevent the laying hold of it with the volsella. But the great pathological contra-indication to its use is found in—2. The *Inflammatory changes*, not only acute, as of any of the pelvic organs, but even chronic, as especially in the cellular tissues. Happily the cases where the volsella would be most likely to cause pain and trouble are also most likely to be diagnosed and treated without its aid, so that we have in them no great inducement to have recourse to its employment.—*Edinburgh Med. Journal*, Oct. 1879, p. 289.

62.—ON INVERSION OF THE UTERUS.

By Dr. LOMBE ATTHILL, Master of the Rotunda Hospital, Dublin.

If the case occur immediately after delivery, the placenta should be at once detached and reposition immediately attempted; but it is otherwise if some days, possibly even hours, have elapsed since delivery; for then the operation becomes very difficult, not, as is usually supposed, from rigidity of the os and

muscular structure of the uterus, but from the very reverse—namely, from the increased softness of the uterine walls. The process of involution of the uterus commences immediately after delivery, possibly, indeed, before expulsion of the foetus, and in a healthy woman proceeds rapidly; one of the first results being increased softness of the uterine walls. The organ is, in fact, undergoing a species of fatty degeneration, which renders the handling of it dangerous. This was forcibly brought under my notice in the following case.

A healthy young woman was admitted into the Rotunda Auxiliary Hospital on the 5th of May last. She had, three months previously, been delivered, after a natural labour, of a healthy child. She was attended by a midwife; but, as far as could be ascertained, no violence or pulling at the funis had been practised. Inversion, however, occurred; and, after the lapse of a few days, she was admitted into the Sligo County Infirmary, under the care of my friend Dr. McDowell. He immediately attempted to effect reposition, but failing, sent the patient to me for treatment. When admitted, she was greatly exhausted, partly from the effects of the long journey, but still more from the constant hemorrhage, which had continued ever since her confinement; and, as there was still a constant oozing, I decided to lose no time in effecting reposition, being the more anxious, as I was at that time under the impression that every day which elapsed would only increase the difficulty of doing so. Accordingly, on the day after her admission, she was brought under the influence of chloroform, and I proceeded to attempt reposition of the fundus, adopting the method which I had successfully practised in a case sent to me from this city (Cork) not long before.

On introducing my hand into the vagina, I discovered that the inversion was so complete, that the lips of the os uteri were undistinguishable; but, on grasping the fundus and making pressure upwards, I speedily succeeded in pushing up part of the cervix, and I was then able to distinguish the rim of the os; this I seized with a vulsellum, with the view of gaining a point of resistance against my upward pressure; but the lip was so soft, that the vulsellum tore through it immediately. I now applied the end of my repositor to the fundus, but soon found that it sank into the uterine tissue. I therefore withdrew it, and tried to effect reposition by pressure on the fundus with the palm of my hand, while with my fingers I pushed up that part of the cervix which had passed last through the os; but, to my horror, my fingers sank so deeply into the wall of the uterus, which seemed as soft as dough, that I believe I must have reached the peritoneum. I at once desisted from any further attempt, and I feared that serious results would follow; the

patient, however, did not suffer the least inconvenience. But I had learned a lesson—namely, that it is both difficult and dangerous to attempt the reposition of the fundus in an imperfectly involuted uterus, and I consequently decided to postpone all further attempts till that process was complete. I accordingly allowed five weeks to lapse, and in the interval I had made for me Dr. J. P. White's repositor, to which I shall allude by-and-bye; and on my next attempt made use of it. I found it to answer very well; and, as the structure of the uterus had become much firmer, I soon succeeded in making the fundus to pass fairly within the os, but, failed, after a protracted attempt, to effect reposition of the inverted fundus. I therefore decided to close the os uteri by means of wire sutures. This I accordingly did, and thus, inclosing the inverted fundus, I hoped that the steady pressure thus exerted on it would have the effect of reducing the size of the inverted mass, and that reposition would subsequently be more easily effected. This practice is recommended by Dr. Emmet, though he prefers the operation of denuding the edges of the os uteri, and thus closing it permanently.

After the lapse of a fortnight, I made a third attempt, and then found, on removing the sutures, that the os uteri had so contracted since the last operation, that I could only get the fingers into it; the inverted fundus seemed smaller and firmer. Having seized the lip of the os uteri with a vulsellum, I proceeded to use pressure on the fundus with a White's repositor, but effected little if any good. The inverted portion had a very peculiar feel; its thickest portion was at the most depending part: here it was about the size of a pullet's egg; from this it narrowed considerably; and the mass felt exactly like an ordinary uterine polypus, with a pedicle rather thicker than usual. It was not, as one would have expected in dealing with an inverted uterus, wider at the base than at the apex, but the very reverse; and the cavity of the uterus was $2\frac{3}{4}$ inches in depth. I therefore felt much doubt as to whether I might not be dealing with a case in which inversion might be complicated by the presence of a small subperitoneal fibroid; the more so as I could not feel any dimple or depression on the surface of the uterus, such as had existed at the commencement of the operation. I believe that, were I dealing with simple inversion, the case was irreducible, and that I would not be justified in prolonging the attempt to effect reduction, which on this the third attempt had lasted an hour. Under these circumstances, I applied the *écraseur* just above the enlarged portion, and removed what proved to be the inverted fundus. The patient recovered rapidly; but, had it not been for the deceptive feel of the part, which led me to suppose that a tumour might possibly

be present, I certainly should not have removed the part, but have adopted Emmet's plan of closing permanently the os uteri, leaving only a small opening to permit the escape of the menstrual fluid. He contends that, if this be done, no hemorrhage will occur, and that, if necessary, the os can be at a future time opened.

At the time when this case came under my observation, I was quite unprepared for the difficulty and danger attending the attempt to effect reposition of an imperfectly involuted uterus. But my first attempt demonstrated this; for, as already stated, my fingers sank at once deeply into the uterine tissue. But since this occurred I have received from Professor White of Buffalo, U.S.A., a copy of his paper on Inversion of the Uterus, in which he fully confirms the opinion I have formed. On this point, he says: "Whilst undergoing this change (*i.e.*, involution), the uterus does not possess the firmness and elasticity of the unimpregnated uterus, nor the muscular flexibility and toughness of that at the full period of gestation. Indeed, I am induced to suspect that, at this period, the uterus cannot be subjected, without danger of laceration, to manipulation which would be perfectly safe at a later period, after complete involution has taken place." I was much gratified and encouraged at finding my opinion thus confirmed.

The conclusion at which I have arrived, then, is this; that if, from any cause, reposition of the inverted uterus be not effected within twenty-four hours after delivery, it is better to delay the attempt for some weeks, till the involution of the organ is completed.

This opinion naturally leads me to the conclusion that mere lapse of time does not materially add to the difficulty of the operation; and this opinion is confirmed by the result of the following case, which has already been published.

S. M., aged 21, was admitted on August 27th into the Rotunda Hospital, on the recommendation of Dr. O'Sullivan, of Cork, who had diagnosed, in the January preceding, the fact that the uterus was completely inverted, the inversion being due to the existence of a fibrous tumour which grew from the fundus. The inversion was complete, and there is reason to believe that it must have existed for quite a year prior to her admission. On August 29th, I enucleated the tumour; and, two months subsequently, I effected reposition with the aid of a very imperfect instrument. In this case, after the lapse of a year, or possibly much more, I effected reposition with little difficulty, having failed in the other case to do so, though but three months had elapsed from the date of her confinement when my first attempt was made.

Dr. J. P. White reports several cases of long standing success-

fully treated by him, and one in which inversion had existed for no less a period than twenty-two years. Clearly, then, time alone is not an important factor in such cases, and only is so where repeated attacks of peritonitis have occurred and dense adhesions formed.

The attempt at reduction being decided on, and the time fixed for the operation, it is next necessary to consider the treatment to be employed. Numerous methods have been suggested; but that advocated by Dr. White is, in my estimation, the best. In my first case, I effected reposition by the use of a very imperfect instrument. It was simply a stem about the thickness of that of a stethoscope, terminating in a cupped disc an inch and a quarter in diameter. But in my second case this instrument failed; and, while I was considering what modifications of it I should make to meet the difficulty, Dr. White's pamphlet, containing a diagram of his instrument, reached me, and I saw at once that it was exactly what I required. I had the instrument made, and found it most satisfactory. The following is his description of the instrument and of his operation.

"By means of the 'repositor,' uniform and gentle pressure can be maintained until the os is fully dilated and the fundus pushed up through it. The insurmountable difficulty heretofore has been supposed to consist in our inability to maintain uniform and persistent pressure for a sufficient length of time. The hand would soon become fatigued, and another hand, even of the same individual, could not be substituted without losing a part of what had been gained. This loss is increased when the hand of a fellow-practitioner is introduced to continue the operation. The various substitutes which have heretofore been resorted to for continuing pressure when the operator has become exhausted have utterly failed. The elastic bags, so often called in requisition, press more upon the viscera resting upon the large surfaces anteriorly and posteriorly situated, than upon the fundus, which has no firm ossific base of support, as have the rectum and bladder. The uterus ascends very soon, owing to the yielding nature of the vagina and perineum, and escapes from the reach of the distended vaginal bags. By means of the large spring at the outer extremity, the amount of pressure can be graduated to an ounce. The disc of this instrument will follow up the fundus, without compressing painfully the urethra or rectum, by means of this continuous elastic pressure in the upward direction, until the fundus disappears in the os and neck. Any intelligent assistant can be trusted to increase or diminish the pressure during the absence of the operator, as the exigencies of the case may demand.

"The construction and action of the 'uterine repositor' will be readily understood by reference to the accompanying woodcuts, Figs. 1 and 2.

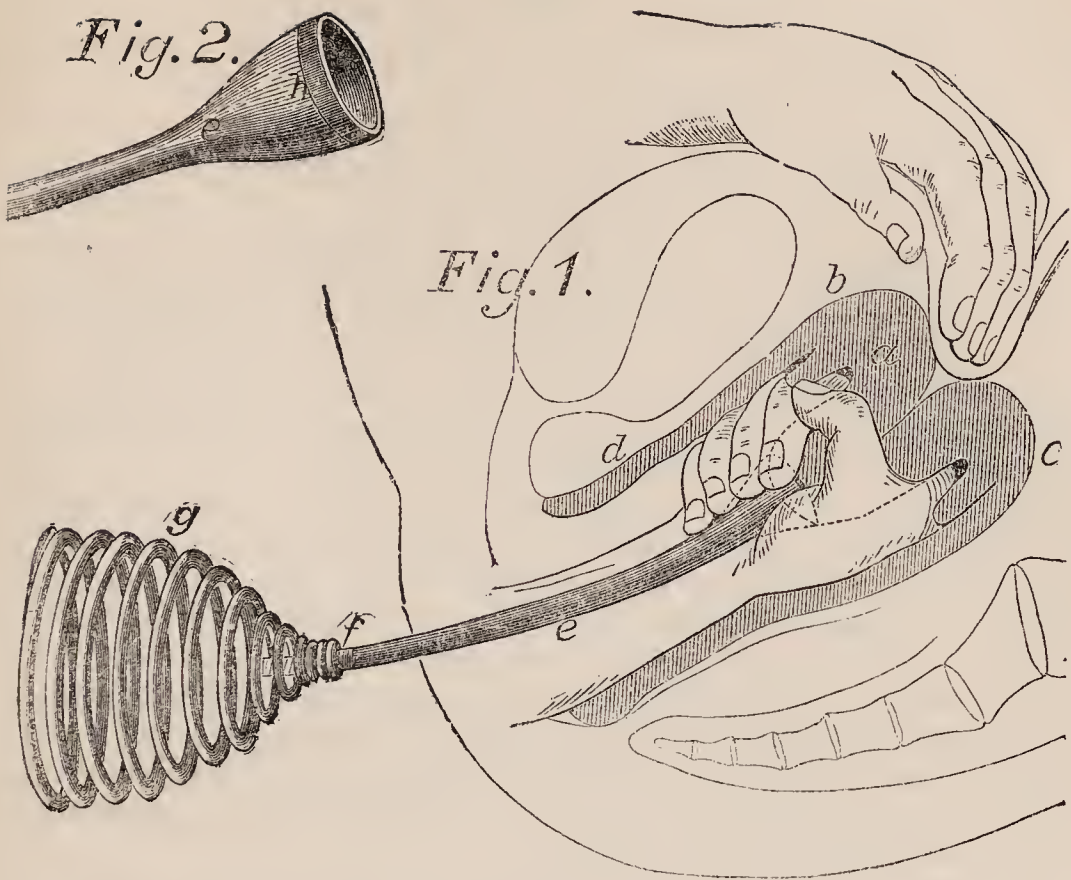


Fig. 1.—*a.* Uterus in process of reduction. *b.* Anterior lip or wall of the uterus with the fingers of the left hand pressing upon it and assisting in pulling open the uterine cavity. *c.* Posterior uterine wall semi-reflected. *d.* Anterior vaginal wall. *e.* Wooden or hard rubber stem of repositor, its enlarged extremity held in contact with the fundus by the intra-vaginal hand of the operator. *f.* Distal extremity of stem made into a screw, so as to be fastened into *g*, a coil of No. 11 steel spring wire, requiring eight or ten pounds' pressure by the breast of the operator, against which it is placed, to bring it down.

Fig. 2.—*h.* Uterine extremity of stem *e*, which is terminated with a soft India-rubber disc $1\frac{3}{8}$ inches diameter.

“The instrument is composed of a stem of wood or hard rubber, curved to conform to the vaginal curvature, with a coil of steel wire attached to the outer extremity, while the other end is expanded and hollowed so as to receive the fundus of the uterus in its concavity or disc. The edge of this disc is tipped with soft rubber, being an inch and three-eighths in diameter and about half an inch deep. The concave extremity of this instrument is carried up into the vagina and placed in contact with the fundus, and then firmly held by the hand in the vagina. The outer end of the instrument, or coil of wire, is placed against the breast of the operator on the same level with the uterus. By means of this large circular spring, the instrument readily keeps its place on the clothing of the operator, and leaves the other hand free to be used above the

pubes to assist in fixing the uterus, and assist also in forcing open the dilating os, which can ordinarily be plainly felt through the abdominal walls.

“The spring at the outer end of the instrument enables the operator, without danger of lacerating the tissues, to keep up a constant gentle pressure upon the fundus, and by leaning forward to increase this pressure intermittingly. The force thus exerted is applied more directly upon the fundus by means of the repositor than would be possible if the thumb and fingers were used, or the round end of the large bougie. I have often been delighted, since I have used the repositor, to find that it gave me a third hand which did not become fatigued, and which permitted me to use the left hand in manipulating over the hypogastrium; while the right easily held the instrument in contact with the fundus, and firmly grasped that part of the uterus which was not yet reflected and which remained in the vagina. The disc, in which the fundus rests, is less likely to bruise or lacerate the organ than any other mechanical appliance. The intravaginal hand compresses the body and fundus, and lessens its vascularity; whilst something is gained by intermitting the pressure, also lessening by its use the exhaustion incident to unintermitting muscular effort on the part of the operator.

“It may be well to state that the patient is always placed at the side of the bed, with the feet resting in the laps of intelligent assistants, each of whom is also charged with the care of the knee and hand of that side. The hips of the patient are brought quite to the edge of the bed, which is raised so as to bring the parts on a level with the breast and arms of the operator.”

I feel convinced, however, that the course I adopt of seizing the lip of the os uteri with the vulsellum, and with it fixing the organ as already described, is very important and greatly facilitates the reduction.

Doubtless, in a few cases, reposition will be impossible; but amputation should, if possible, be avoided; and I therefore think Dr. Emmet's suggestion, if the fundus can be pushed up sufficiently to permit its being enclosed inside the os, of paring the edges of the os and permanently closing it, is well worthy of being practised. It is certainly a proceeding much safer than amputating the fundus, and, as Dr. Emmet points out, does not absolutely render pregnancy impossible; for an opening must be left to permit the exit of the menstrual fluid, through which impregnation may possibly take place.—*British Medical Journal*, Sept. 6, 1879, p. 357.

63.—ON THE TREATMENT OF CHRONIC COMPLETE INVERSION OF THE UTERUS ;

WITH CASES ILLUSTRATING THE ADVANTAGES OF THE DIRECT METHOD OF APPLYING ELASTIC PRESSURE.

By Dr. J. H. AVELING, Physician to the Chelsea Hospital for Women.

During the reposition of an inverted uterus, it may be made to pass through three distinct movements, the results of three methods of reposition—fundal, lateral, and cervical.

Fundal Reposition is impossible except in cases of acute inversion, when the walls of the uterus are very much relaxed. It is attempted by pressing upon the fundus with the fingers in the form of a cone, or with a drumstick-shaped instrument. The object is to drive the fundus through the cervix. It is the most unscientific method of replacing an inverted uterus, as it demands unnecessary dilatation of its neck. Fundal reposition ought never to be employed ; it has been superseded by

Lateral Reposition.—This is a very effective plan of reducing acute inversion. The secret of its success has been well explained by Dr. Marion Sims. “By this means,” he says, “we slide one-half of the organ at a time through the os internum, instead of the whole fundus, which presents a greater diameter.” The manœuvre is performed by the hand, pressure being exerted against either horn of the uterus. Some years since, I was requested to remove what had been diagnosed as a polypus, but which turned out to be an uterus inverted after the patient’s confinement ten days previously. In this case, I first tried the fundal plan, but without success. The lateral method succeeded in reinverting the organ in two or three minutes.

Cervical Reposition.—It is difficult to say to whom the credit of having first recommended this plan of reposition is due. In this country, Newnham drew attention to it as early as 1818. He says: “It is wisest and best to endeavour to reinvert the uterus by returning first that portion of it which was last expelled from the os uteri” (Essay on Inversio Uteri, p. 16). The recognition of this fact was an immense step in advance, and upon it depended the success now reached. Cervical reposition is produced by pressure upon the fundus ; but all idea of indenting it is discarded.

Rate of Reposition.—Whether we look upon the uterus as a plastic organ to be moulded, or a muscle to be tired out, it must be equally unscientific to endeavour to force it to change its form suddenly. When it has been inverted two or three months and the process of involution has been completed, rapid reinversion, whether it be attempted by hand or by the repositor, is an operation unnecessary and unjustifiable. There is no need for

such dangerous haste. It will probably be found that even gradual pressure had better be exercised intermittently rather than persistently; for the continuous forcible urging of the cup of the repositor against the fundus may result in sloughing.

Aids to Reposition.—Many adjuvants to reposition have from time to time been suggested and employed. *Posture, hot-water injections, belladonna, and galvanism* have been recommended. *Compression* of the uterus by hand, forceps, or bandage, has been also used; but it can be of no use in chronic inversion. It belongs to the past period of fundal reposition, and is not free from danger, as in some cases it has caused sloughing. *Dilatation* of the cervix is another mode of aiding reposition. It may be effected by the fingers acting through the vagina, the rectum, or the abdominal walls. It has also been performed by opening the abdomen and passing a dilating instrument directly into the cervix. *Incisions* have been recommended for the purpose of overcoming the rigidity of the cervix; but they have not been found to answer in all cases; and, where they have, lacerations have also occurred, extending the lesions caused by the knife. *Sutures* have also been passed through the neck of the uterus to maintain the ground gained when reposition has been half performed. Fortunately, it may now be stated that all these aids to reposition, whether they be harmless or dangerous, will henceforth be unnecessary.

Repositors.—Various instruments have been used from time to time for the purpose of replacing inverted uteri. Of these, the most important are the hand, elastic bags, straight rods with round and cupped ends, curved rods with round and cupped ends, and double curved rods with cupped ends.

The Hand has been much used as a repositor in chronic inversion; but, it must be confessed, more often with the result of lacerating the vagina than reinverting the uterus. It is an admirable instrument in acute inversion, when lateral reposition is so desirable; but in chronic cases it is cruel and inefficient. Time is now required. No hand can sustain the fatigue of taxis for a sufficiently long period. It is most unscientific to demand suddenly from the uterus that which it will willingly grant if reasonable time be only afforded. The hand, therefore, must be discarded as a repositor in chronic inversion.

Elastic Bags have many faults. They act too slowly; they compress parts which need no pressure; they slip out of the vagina, obstruct the action of the bladder and rectum, and lack precision in exerting their power. It was upon the elastic bag, however, that we took the first great step towards perfecting the application of gradual elastic pressure in the cure of chronic inversion; and to our countryman Dr. Tyler Smith undoubtedly belongs the credit of having done this service. Elastic bags have cured many cases of inversion; but their day has passed.

Straight Rods with Rounded Ends, like drumsticks, were the first instruments employed. They were used for indenting the fundus, and thus reinverting the uterus by fundal reposition. No one dreams of using such a repositor now.

Straight Rods with Cupped Ends.—Perhaps the first cupped repositor mentioned is that referred to by Madame Boivin. In Hemming's translation, page 126, we find the following remarkably suggestive passage. "Might not pressure from below upwards be also made available? A pessary, like that of a cup and ball, might, for this purpose, be introduced into the vagina." The change from the convex to the concave extremity of repositors was a great advance, and, in fact, became inevitable as soon as operators abandoned the fundal and adopted the cervical method of reposition. The straight cupped repositor is nearly a perfect instrument; but it lacks the power of applying pressure to the fundus in the most direct and efficacious manner. It must always act laterally, causing the cup either to slip past the fundus or to urge it against the posterior wall of the vagina in a line diverging 25° from the axis of the pelvic inlet.

Case 1.—A successful case treated with a straight cupped repositor by Dr. Matthews Duncan (St. Bartholomew's Hospital Reports, vol. iv., p. 97) will illustrate its advantages and disadvantages. The main facts are briefly as follows. Mrs. S. A. C., aged 38, was admitted into St. Bartholomew's Hospital on March 13th, 1878, with chronic inversion of the uterus, caused by a fibroid. This was removed, and the treatment for reinversion commenced on March 16th by "persistent and very powerful reducing taxis," employed by Drs. Duncan, Godson, and Champneys, but in vain. On May 21st, a straight cupped repositor was applied, and elastic pressure maintained for seventeen hours. Pain, vomiting, and distress rendered it necessary to remove the instrument. On May 28th, a second attempt at reduction was made, but "*failed from the cup slipping off the fundus uteri*." On June 4th, the repositor was again used, and in seventy-four hours reposition took place.

Curved Rods with Round Heads.—Chailly Honoré mentions the use of a "*bâton courbe renflé à son extrémité*" in cases of inversion, and White's first repositor was of this form. Round-headed rods with a pelvic curve are not now used.

Curved Rods with Cupped Ends.—These are not such perfect instruments as straight cupped rods; but they are still much in favour with some gynecologists. They exert pressure in a wrong direction; that is to say, in a line diverging as much as 45° from the axis of the pelvic inlet. Consequently, all the disadvantages of a straight cupped repositor are increased.

Case 2.—A case treated by the curved cupped repositor, under the care of Dr. Robert Barnes (Obstetrical Journal, vol. i., p. 5),

may be given in illustration. It is briefly as follows. H. H., aged 27, was admitted into St. Thomas's Hospital February 19th, 1872, suffering from chronic inversion of the uterus.—February 20th. Supposing the uterus to be a polypus, the *étraseur* was applied; but, pain being complained of, it was removed.—March 6th. Inversion having been diagnosed, the “largest hydrostatic dilator” was applied in the vagina and used for two days.—March 13th. A curved repositor was inserted and retained until the 19th, when pain, vomiting, and rigors came on, and the instrument was removed.—March 22nd. The patient was placed under chloroform, and two incisions were made in the cervix uteri, and reduction by taxis attempted. Failing, the repositor was again employed, and continued in use until April 9th, having been temporarily removed on the 5th and 6th, while further attempts were made by taxis. On the 9th, the patient was greatly troubled with vomiting and prostration, and it was determined to make another attempt at reduction by taxis. This was the fifth attempt to reinvert the uterus by hand, and it succeeded.

Double Curved Rod with Cupped End.—It will have been noticed that neither the straight nor the single curved repositor exerts pressure in the most advantageous direction. The necessity of doing this has been long recognised. Burns, in 1728, says reposition “may be facilitated by pressing up the fundus in the direction of the axis of the uterus”; and Aitkin, in 1784, remarks that reinversion may be best “effected by pressure in a just direction.” Gooch, in 1831, gives us the most explicit instructions. He says: “You are to press it (the uterus) against the os tincæ, not upwards and backwards, but in the direction of the upper axis of the pelvis, upwards and forwards towards the præcordia.” Nothing more of a practical nature relating to the line in which force should be used can be added to these words. In inventing my repositor, I have endeavoured to construct it in such a form as to make it possible to carry out Gooch's instructions. The rod which supports the cup has been doubly bent, and has a pelvic and a perineal curve. With this instrument, the fundus of the inverted uterus may be pressed “*upwards and forwards*” in a direct line with the axis of the uterus and the pelvic inlet. Dr. Matthews Duncan, when detailing his case, has referred to my instrument. He says: “It has been thought advisable by Aveling to give the stem of the repositor a perineal curve, such as is observed in Tarnier's midwifery-forceps, and for reasons analogous, if not identical. But such modification of the simple straight repositor seems to me unnecessary.” I take this opportunity of explaining that the curve in Tarnier's forceps, from which I am supposed to have derived my idea, was invented by myself and applied to my forceps and published ten years

before Tarnier made his forceps known. When I published the first notice of my repositior, I said the straight repositior must have a tendency to slip past the fundus; and in the next paragraph to this, in which Dr. Matthews Duncan expresses his satisfaction with the straight instrument, he records the occurrence of the very accident I predicted.

My repositior has been used three times during the past year—twice by myself and once by Dr. John Williams.—*British Medical Journal*, September 6, 1879, p. 359.

64.—ON METRORRHAGIA, AND ITS TREATMENT BY PLUGGING THE CERVICAL CANAL.

By Dr. HENRY BENNET, The Ferns, Weybridge.

In the Journal for July 19th, the Paris correspondent, in reporting a discussion on the treatment of menorrhagia, which took place at the Clinical Society of Paris, presents as a novelty the proposal of M. Panas to meet severe metrorrhagia by plugging the cervical canal itself, instead of the vagina. As I have been in the habit of resorting to this treatment for above a quarter of a century, and believe that I was the first to suggest and describe it in the fourth edition of my work on *Uterine Inflammation*, published in 1861, I send a brief extract (page 422), not so much from a desire to claim the priority of what really is a most valuable addition to therapeutics, but because I wish to draw the attention of the profession to its thoroughly practical character.

“In those cases in which, as we have seen, the hemorrhage persists after the entire removal of local disease, owing to enlargement of the uterus, to the presence of a small unrecognised polypus or uterine tumour in the cavity of the uterus and its neck, or to the mere hemorrhagic habit, I have for many years resorted, with encouraging success, to plugging *the os uteri itself*, instead of the vagina. It occurred to me that the usual plan of filling up and distending the vagina by pieces of sponge or a handkerchief was a clumsy, painful, and inefficient mode of opposing mechanical resistance to the exit of blood from the undeveloped uterus, when its orifice could so easily be brought into sight. Acting on this idea, I have in many instances brought the cervix uteri into view, and passed inside the os two or three small pieces of cotton, tied to a piece of thread, which I wedge in firmly, covering the whole cervix with two or three larger pieces left in close contact with it on the withdrawal of the instrument. In nearly all the cases in which I have resorted to this plan, I have easily arrested the hemorrhage. Indeed, this modification of the usual plan appears to

me so simple and so consonant with common sense, that I cannot but think that it will be often adopted in severe cases. In the ordinary operation of plugging the vagina, that canal has to be distended by a large mass of sponge or linen, soaked with clotted blood, which often interferes with the functions of the bladder and rectum, is invariably a source of great discomfort to the patient, and is not always efficient. By the plan I describe, the end proposed is much more effectually compassed, with scarcely any annoyance to the patient beyond that which the use of the speculum occasions.

“Owing to the natural contractility of the cervical canal, and the pressure of fluids behind, if the cotton be not well pushed in it is soon forced out. The plug may be left without renewal twenty-four or even thirty-six hours; but, in the latter case, it is generally expelled spontaneously. A small piece of sponge may be used, and is more likely to remain *in situ* owing to its expansion; but as it must necessarily be very small, it is more likely to be permeated by blood. If sponge be used, great care should be taken to extract the piece passed into the os, to which a small piece of thread should always be tied, as the os uteri might not be able to expel it alone, owing to its mode of expansion.

“Plugging the os uteri in the way described is by far the most effectual way of arresting the hemorrhage which precedes and accompanies abortions, when, as sometimes occurs, it resists the usual treatment, and becomes alarming. The following case will illustrate this mode of treatment and its marvellous efficacy. A healthy lady, thirty-two years of age, came up from the country to consult me. She had been married twelve years, had had five living children, and subsequently four miscarriages. Each miscarriage had been attended with gradually increasing hemorrhage. On the last occasion, she nearly lost her life, and was greatly alarmed at the idea of again becoming pregnant. She presented uterine symptoms which led me to discover the existence of extensive ulcerative disease of the cervix uteri; no doubt connected with one of her pregnancies, and the cause of the miscarriages. I cured the disease, and sent her home. A few months afterwards, she became pregnant; and, in her alarm, again came up to town to be near me, although I rather dissuaded her from the step, which I did not consider necessary. At about the termination of the third month of pregnancy, I was sent for one night with the intimation that flooding had set in. I went immediately; and, on my arrival, found the patient pale, all but pulseless, and lying in a large pool of blood. The hemorrhage had commenced at half-past twelve; it was only two, and yet her state was alarming; the hemorrhage was continuing profusely, and the loss had

already been very considerable. I at once placed the patient on her back, to facilitate manipulation (with half a dozen candles on a table at the bottom of the bed), introduced a large conical speculum into the vagina, and, with the speculum-forceps and large pieces of sponge, rapidly cleared the speculum, which was full of clots and fluid blood up to the brim. This accomplished, I got the os uteri into view; found it rather open; from its orifice a stream of blood was flowing as rapidly as from an open vein. I at once pushed in several pieces of cotton tied to threads, holding one down by the sound, whilst the next was being introduced, to prevent it from being washed away, and then packed firmly the upper third of the speculum with cotton around and over the cervix. Lastly, I slowly removed the speculum, forcibly supporting the cotton packing inside with the forceps to prevent its being displaced. I had the mass of blood in which she was lying removed. In the course of about twenty minutes, the colour began to return to the face, and the pulse began to be more perceptible, and, in a few hours, she was nearly herself again, although very weak. There was not subsequently the slightest hemorrhage; it was permanently checked. Twenty-four hours after, I removed the plugging, and did not reapply it. No fresh loss of blood was experienced; and the next day, after a few uterine pains, a diseased ovum was expelled. This lady afterwards rallied in a few days. After the last previous abortion, she was ill for months."

The above extract from my work (1861) gives so full and complete an account of this mode of treatment, and of the manner in which I usually carry it out, that there is very little to add. I would remark, however, that I use dry cotton only, and no styptic or caustic solution whatever. The perchloride of iron is totally uncalled for, unnecessary; for the plugging is in the cervical canal, and the hemorrhage all but always proceeds from the uterine cavity. The treatment is as simple as corking a bottle. If the cork be well pushed in, no water (blood) can come out; the hemorrhage is arrested in a few minutes, and the patient rallies all but immediately. I have had many similar cases since this was written, and no longer fear hemorrhage in the undeveloped or slightly developed uterus. The process also appears to be entirely devoid of risk or danger. I have never had a bad symptom. The same cannot be said of uterine injections. The wife of one of my colleagues at Mentone died of tetanus after the injection of perchloride of iron into the uterus, to arrest hemorrhage in early pregnancy. She would probably have lived, had plugging of the cervical canal been resorted to as above.—*British Medical Journal*, July 26, 1879, p. 122.

65.—ON OVARIAN MENORRHAGIA.

By Dr. ALFRED MEADOWS, Physician-Accoucheur to St. Mary's Hospital, and Lecturer on Midwifery at the Medical School.

It may be well, perhaps, before I speak on the subject of my paper, that I should explain briefly what I mean by the term "ovarian menorrhagia," because, so far as I know, it is not one which is in common use, nor indeed is it, I believe, to be met with in works devoted to the subject of diseases of women; and yet, as I hope to be able to show you, it is a term peculiarly applicable to the class of cases we shall have to consider—a term, also, of much practical value, inasmuch as it not only indicates distinct physiological teaching, but also defines a particular pathological condition; and, moreover, it serves to point out the special line of treatment which ought in the main to be pursued in the cases to which it refers.

Let us pause for a moment to look at the conditions and circumstances of ordinary natural menstruation; for without this we shall not understand or realise the fitness of the term here employed as the title of my paper. First, then, on the one hand, we see certain changes going on in the ovary *precedent* to certain other changes which take place in the uterus. The former are characterised by great vascular excitement; the vessels are turgid and swollen, and the organ itself is much increased in size, especially at the point of chief functional activity. All these changes are preliminary to, or coincident with, the maturation and escape of an ovum from a given point on the surface of the ovary; and they all precede in the order of occurrence the changes which take place in the Fallopian tube and uterus. These latter changes are also characterised by great vascular activity. Here, also, the vessels increase in size; the mucous membrane, both of the Fallopian tubes and uterus, becomes greatly swollen, and a kind of erection takes place, to which, if I may venture to hazard the opinion, is due the application of the fimbriated extremity of the Fallopian tube to the surface of the ovary, in order that it may receive and conduct to the uterine cavity the ovum which is to escape from the ovary, for the purpose of either impregnation, or that it may be cast off as "the untimely fruit of the woman." Now, the prominent feature, characteristic of, and resulting from, the changes which take place in the Fallopian tube and uterus, which changes are evidently due to the fact that these latter are lined by a highly vascular mucous membrane, with all that that fact implies, is the escape and discharge of a variable quantity of mucus and blood, accompanying which there are also the minute disintegration, death, and shedding of the mucous membrane itself.

Such, in very brief terms, are the leading phenomena attendant upon ordinary menstruation; and it will be admitted, I think, upon a review of them, that the ovary is, as it were, "master of the situation"—that it is, in fact, as I have often insisted, highest in physiological dignity and importance, and that the uterus, so far as menstruation is concerned, merely obeys the physiological behests of the ovary. How, or in what way exactly, that is done; what the minutely intimate process by which the special functional activity of the ovary, which we call ovulation, produces or eventuates in menstruation, is a problem which yet remains to be solved. The fact, however, is indisputable; for without ovulation there is no menstruation. We may, I think, go one step further, and say that, whatever be the exact nature of the process, it is, in all probability, wrought through the instrumentality of the local nervous system; and that this is so, another fact, to which I shall call your attention presently, testifies; namely, that the medicines which possess most therapeutical value in the treatment of the special class of cases which we have to consider to-night are known to exercise their influence chiefly, if not entirely, through the nervous system.

In the practice of medicine, as in some other concerns of life, we are very apt unconsciously to be influenced by the terms we use to denote ideas; and hence the employment of the term "uterus and its appendages" has led us to regard the former as the most important part of the female generative system, both as regards physiology and pathology; whereas in reality the ovary, which is one of these so-called appendages, is from both points of view superior in importance and interest, as dominating, if I may so say, the functions, as well as influencing the diseases of the uterus.

Admitting all this, then, if it be true that the ovary is so important a factor—nay, is *the* factor, in the physiological occurrence of menstruation—does it not follow that, at least in some cases, and probably in many, the ovary is *the* factor, also, in the various derangements to which that function is liable? We know that it is so in regard to one derangement—namely, pain; for we speak constantly of ovarian dysmenorrhœa, meaning thereby menstruation made painful by reason of ovarian disease of one kind or another. But if the periodical discharge of mucus and of blood from the uterus, which we call menstruation, be the direct result of ovarian action, is it not reasonable also to suppose that the *amount* of that discharge may in like manner be governed entirely by the character of that ovarian action; in other words, that just as scanty menstruation, amounting in some cases to a mere show, lasting only for a few minutes, and being sufficient merely to stain a small piece of linen, is the

direct consequence of feeble ovarian activity, due possibly to an almost infantile, and certainly to a very undeveloped, form of ovary; so, on the other hand, excessive menstrual discharge—menorrhagia, in fact, and amounting, it may be, to a positive hemorrhage—may in like manner result simply from ovarian activity of quite another kind, and be the direct consequence of morbid and, may I not add, excessive ovarian action? Nay, further, does not the physiology of menstruation seem to show, also, that cases of menorrhagia in regard, not to the amount of menstrual discharge in any given “period,” but to its frequent periodicity, its too frequent occurrence, in fact, are also the direct results of too frequent ovarian action—in other words, of too frequent ovulation? All this, I submit, is fair reasoning; and, moreover, it is borne out by careful study of clinically observed facts; for the cases are many and of frequent occurrence in which the most careful examination of the uterus fails entirely to demonstrate any morbid condition of that organ, while minute enquiry equally fails to bring out any tendency to the so-called hemorrhagic diathesis; and yet menorrhagia, either in time or in quantity, is the prominent feature in the menstrual act of these persons.

Furthermore, be it observed that, if attention be directed away from the uterus, and towards that other organ which we are specially considering to-night—namely, the ovary—we shall find, on inquiry, that symptoms clearly referable to that part exist to a marked degree. For instance, as a general rule, pain, to a greater or less extent, is a constant feature in these cases, and it presents the following characteristics. It is situated in one or other ovarian region, much more frequently in the left than in the right side; why this is so, I cannot tell; but, in my experience, morbid conditions of the ovary in which the blood-vessels are chiefly involved are far more common in the left than the right side. It is the same, I believe, in the male subject; varicocele is more frequent in the left than in the right testis. I am not sure whether the same rule obtains in regard to orchitis. You will remember the difference which exists in the anatomical arrangement of the blood-vessels—the spermatic artery and vein—of the two sides. Whether this is related to the fact just referred to I am unable to state, though it seems not unlikely. Again, this ovarian pain extends upwards to the renal region, and, above that, under the ribs of the corresponding side, to the mamma of that side, which often becomes very painful, and even tender to touch. In these respects, it differs from the pain which originates in one or other broad ligament; for this goes through the sciatic notch, and down the thigh, both back and front of the affected side; it does not extend upwards at all. In the case of the ovary, the pain is referred upwards along the

spermatic plexus, which, as you know, is formed from branches of the renal and aortic plexuses; and it is by reference to these anatomical facts, of the relation of the spermatic to the renal plexuses, that I would venture to explain another symptom, common not only to the cases we are considering, but met with frequently also in other ovarian affections—I refer to the existence of the so-called hysterical urine. I believe there is a very close analogy in the mode of production of the so-called hysterical urine, which is an excessive secretion of the kidney, and these cases of ovarian menorrhagia, which is an excessive secretion of the uterus. At all events, the coincidence is often very remarkable.

Again: women who suffer from ovarian menorrhagia are frequently subject to loss of voice, to difficult deglutition, and other throat affections. I need not mention the *globus hystericus* with which you are all familiar. It would be an interesting subject for discussion, if I had time to consider it, which I have not—namely, the relation of the parts about the throat, the larynx, etc., and the generative apparatus, but especially the ovaries, in women. Not that the human animal is the only one, or the female the only sex, in which this relation exists; for at this period of the year, when the birds are coming into song, we are audibly and pleasantly reminded of this relationship.

Lastly, and this is the climax in the clinical study of these cases, when we institute a vaginal examination for the purpose of discovering the cause of the menorrhagia, which is frequently very excessive, amounting, as I have said, in some cases to a positive hemorrhage, while in others the amount discharged at any given time is perhaps not very, or not at all, excessive, but is far too frequent; it may be fortnightly, or more rarely weekly, or even almost constant, to a greater or less extent, as the case in one under my care; when, I say, an examination made, in the expectation, it may be, that either a polypus or some other organic disease of the uterus exists, or that the cervix uteri, the seat of the so-called granular ulceration, or that the uterus is abnormally large, the os patulous, and other conditions existing to indicate the state of subinvolution, we may be surprised to find, if we have not inquired carefully and minutely into the clinical history of the case, that none of these conditions exist—that, in fact, the uterus is perfectly healthy, that only negative evidence is furnished by examination of the organ, and that we must look elsewhere if we are to discover the real cause of the mischief, and if we are therefore to apply our remedies intelligently and successfully in the treatment and cure of the case.”

Let me now say a few words as to the treatment of these cases of ovarian menorrhagia. I do not think I at all exag-

gerate when I say that, in ninety-nine out of every hundred cases of menorrhagia which come before the practitioner for treatment, his first thought is, What form of astringent shall I give? And the answer probably in most cases will be, an astringent chalybeate—either the perchloride or the perntrate, or some similar preparation of iron, will be almost certainly prescribed. No wonder that such routine practice frequently fails; for, as I stated at the beginning of my paper, a very considerable number of cases of menorrhagia which come up for treatment are of the kind we have been considering to-night, and for such as these, the persalts of iron are worse than useless; their only effect will probably be to still further force on ovulation, and thus to aggravate the complaint. Indeed, if the pathology which I have sought to enforce in this paper has any basis in reason and fact, then to administer astringents in any form is surely unscientific; it is, as it were, beginning at the wrong end; it seeks to curtail capillary action at the uterine end of the pole as it were, whereas in reality, the mischief is being wrought at the ovarian end; and our treatment, therefore, ought to be directed to this point. In other words, we should disregard the result—the menorrhagia, and we should concentrate our attention upon the cause—the ovulation. Hence the remedies ordered in the above mentioned case, and hence the successful issue.

Of all drugs in the Pharmacopœia, I know of none which possesses such great power as the bromide of potassium in controlling this particular form of menorrhagia. I ought rather to say, for that better expresses my meaning, that no other drug possesses in a like degree the power of limiting ovulation. I believe, indeed, that we may absolutely suspend the function altogether, and produce in time an atrophy of the ovary by the prolonged administration of this drug in large doses. I have seen cases again and again in which menstruation, and therefore ovulation, has been delayed for weeks from apparently no other cause than this; and I have successfully arrested and cured scores of cases of this form of menorrhagia without ever giving any kind of astringent, but merely administering the bromide of potassium. Occasionally, but by no means always, I combine with it the bromide of iron. This drug seems to me to possess a somewhat similar action; certainly it does not act in any way as the other salts of iron. It seems to favour the absorption of certain glandular swellings, which cannot be said at least of any persalt of iron. Sometimes I give the iodide of potassium, and sometimes the iodide of iron, in conjunction with the bromides. I believe that they all act very much in the same manner; but certainly none are so efficacious as the bromide of potassium. Indeed, I may say it is my sheet-

anchor in these cases, and I regard it as almost, if not quite, a specific in ovarian menorrhagia. What its special mode of action may be, I fear I cannot with certainty divine; there is, indeed, nothing more difficult to define than the exact *modus operandi* of drugs; but I would at least venture to state my conviction that this drug acts directly through the nervous system, and especially upon the nerves of the blood-vessels. In this way it is, I believe, that it exerts so beneficial an influence in cases of epilepsy; and it seems to me not unlikely that the good results wrought in those cases of epilepsy in women, which occur so frequently in connection with menstruation, are due directly to the influence of the bromide in controlling ovulation, and so diminishing this form of reflex irritation.

In connection with the hypothesis here stated as to the *modus operandi* of bromide of potassium upon the nerves of the ovaries, let me refer to the other part of the prescription given in the case above detailed. The two alkaloids—conia and atropia—were ordered to be used *per vaginam*. You will remember that pain became a prominent symptom in this case after the patient married; and I have just stated my belief that the bromide acts beneficially upon the local nervous system. It would seem, then, if this be therapeutically sound, that we ought to seek to influence nervous action in order to control ovulation. And this would appear to be specially necessary where pain was also a prominent feature. Now, of all the anodynes we possess, none, I think, can compare with conium as an anodyne to the generative or sexual organs; and its influence upon the ovarian nerves is quite remarkable. I have even thought, from observations I have made, that it not only allays pain, but that it also calms vascular excitement, and so exercises a controlling and moderating influence even upon ovulation itself. Nor does this seem unreasonable or unscientific, when we consider how much vascular activity is influenced by nervous excitement. At all events, I am satisfied that it does exercise a most beneficial effect in the class of cases we are considering. Atropia or belladonna seems also to possess similar powers, but by no means to the same extent; and it has the disadvantage of causing sometimes serious constitutional disturbance even in small doses. Moreover, with an agent so efficacious, and I may add so uniform, in its results as conium, we need not multiply our resources. I may say that I always use the alkaloid *conia* in one-grain doses for a pessary, just as I prefer atropia to belladonna, because the alkaloids are cleaner in use, do not create any dirty-coloured discharge, are smaller in bulk, and are certainly not less effective. —*British Medical Journal*, July 12, 1879, p. 45.

66.—ON THE USES OF IPECACUANHA IN UTERINE HEMORRHAGE, AND DURING PARTURITION.

By the EDITOR OF THE MEDICAL PRESS AND CIRCULAR.

We have long considered the many therapeutic virtues of ipecacuanha to be less widely known than they ought to be. Everyone knows that this drug is an emetic and expectorant, but few are so well acquainted with its contra-stimulant, diaphoretic, cholagogue, purgative, and anti-hemorrhagic properties. The wonderful effect of large doses of ipecacuanha as a cholagogue purgative, and in the treatment of dysentery and certain diseases of the liver, has been well demonstrated by Dr. Maclean, whose experience of this drug in India has been very great. But of its anti-hemorrhagic and oxytocic properties the profession generally have but little knowledge. We were, therefore, struck with an able article which appeared recently in the New York Medical Journal from the pen of Dr. John H. Carriger, of Tennessee, on the influence of ipecacuanha in increasing the expulsive contractions of the uterus, in facilitating the dilatation of a rigid os, and in arresting uterine hemorrhage. Its power of checking hemorrhages, especially from the womb, has been acknowledged by many writers on materia medica and therapeutics. Dr. W. Chapman ("Elements of Therapeutics," Philadelphia, 1821) says that the powers of small doses of ipecacuanha are frequently astonishing in checking uterine hemorrhage. Dr. Osborne, of Dublin, states that the treatment of menorrhagia by emetic doses of ipecacuanha has never yet failed in his hands. Braithwaite (*Retrospect*, July to Dec., 1845, p. 172) states that John Higginbottom, in profuse uterine hemorrhage, succeeded with it when ergot failed. Dr. J. Eberle ("Materia Medica and Therapeutics," Philadelphia, 1842) observes that in uterine hemorrhages it is more decidedly beneficial than in other hemorrhages. Mr. Trenor (Dublin Journal, vol. xviii), goes so far as to say that nauseating doses of ipecacuanha were, in numerous cases under his care, attended with marked benefit, the hemorrhage being arrested, and heat and life restored to patients who were in a state of collapse from excessive loss of blood. Dr. Graves ("Clin. Lect.," vol. iii., p. 141), who observes that Richter was the first to point out the anti-hemorrhagic effect of ipecacuanha, placed great confidence in nauseating doses of the drug, both in hemorrhage from the stomach and from the bowels. He did not think this effect was due to its nauseating properties, but to some specific action. Its influence over uterine hemorrhage and menorrhagia, Dr. Tyler Smith explained in the following manner: "Ipecacuanha," he says, "by its emetic properties, excites contraction of the abdominal muscles and compression of the uterus, which

may in turn re-excite some amount of reflex action, but beyond this it appears to have a special action upon the uterus, increasing its contractile power beyond what could be imagined to occur from the merely secondary effects of vomiting."

It is however, of its utility during parturition that Dr. Carriger has had most experience—an experience extending over several years. He believes that its power over uterine hemorrhage is due to its power of co-ordinating uterine action and stimulating tonic contraction. He believes that in ipecacuanha we have an oxytocic, potent and safer than ergot for both mother and child, because it stimulates the uterus into a more normal action, and at the same time facilitates dilatation of the rigid os. Dr. Carriger cites several cases in which the administration of two or three grains of the drug was followed by relaxation and dilatation of the os, great increase of the uterine contraction, bearing down pains, and the safe and speedy termination of the labour. In one case, after the exhibition of two grains of the drug, a labour, at first believed to be impossible (owing to some contraction at the brim), was by the natural efforts alone safely terminated in less than four hours. "I might add," he says, "case upon case in which, during the past twenty years, I have thus used ipecacuanha at some period of the labour, in all of which the result has been the same—a great lessening of the sufferings of the patient, and a shortening of the duration of labour." Again he observes that in about thirty minutes after taking the ipecacuanha the restlessness, rollings and tossings, of the patient cease, she becomes quiet, is sensible of the favourable change, the os dilates, the child descends, and the case is soon over, and yet the labour does not possess any of the characters of an ergotised labour, the pains being more regular, more natural, and succeeded by intervals of rest. Moreover, in a very large number of cases of rigid and undilated os, where every pain, for hours previously to its administration, had been accompanied by loud outcries, jactitation, irritability of temper, &c., this drug has rarely failed in a few minutes to cause marked change in the condition of the patient, and to conduce to the speedy termination of the labour. It is to its power of causing relaxation and dilatation of the os, that Dr. Carriger is inclined to attribute the influence of ipecacuanha over the sick stomach of pregnancy—an opinion which receives some corroboration from the fact that some obstetricians of the present day have found certain cases of obstinate vomiting to depend upon a morbid condition of that part of the womb.

It may be that Dr. Carriger has been induced to form a higher opinion of the oxytocic action of ipecacuanha than will be warranted by future experience. At the same time the

results which he appears to have obtained from the administration of this drug during parturition deserve the careful consideration of all who are anxious to add to our means of lessening the delay, the pains, and the distress which so frequently attend the first stage of tedious labours.

There are occasions when neither chloroform, nor opium, nor chloral, is indicated, or admissible; or when, if admissible, they fail to have the desired effect. Chloroform may render the patient insensible to the pain, but it has little influence in relaxing the os, and rather backens than shortens the labour. To opium, many objections may be made; while chloral is simply a soporific. Nauseating, or emetic doses of tartar emetic, used to be given in some of the cases for which ipecacuanha is here recommended; but the action of that drug is too violent, too depressing, and too prolonged to make it, in these circumstances, a safe and convenient remedy. Whether or not Dr. Carriger has exaggerated the utility of this drug in cases of tedious labour we shall not presume to say; but at all events it is a question that might be very soon decided. Half-a-dozen practitioners, with large midwifery practices, might settle it in as many months; and it would be certainly worth while giving a fair trial to this alleged efficacy of ipecacuanha as an oxytocic remedy. There cannot be the slightest doubt that hundreds of women are allowed to endure much unnecessary pain and suffering during the first stage of labour through want of some agent that will at once facilitate dilatation of the os, and increase the expulsive force of the uterus itself. That agent may not be ipecacuanha, but if not, it remains to be found; for none of the drugs above mentioned can be said to have supplied this desideratum.—*Medical Press and Circular*, May 21, 1879, p. 409.

67.—NOTES ON INTRA-UTERINE MEDICATION.

By Dr. G. GRANVILLE BANTOCK, F.R.C.S., Senior Surgeon to the Samaritan Free Hospital for Women and Children.

Ever since the practice of applying fuming nitric acid was advocated for the treatment of chronic uterine catarrh, I have looked forward, in confident anticipation, to the time when we should hear of the dire consequences of such heroic treatment. Now my expectations are realised. Holding these views, it will not be expected that I can afford any direct evidence, for I have never employed the pure acid, and it will be readily understood with what horror and amazement I have listened to gentlemen declaring that they had used it in *hundreds* of cases.

Now I, for one, cannot but regret that a method of treatment—viz., intra-uterine medication—which yields such satis-

factory results, when judiciously employed, should be thus brought into discredit; for I can fully confirm the statements of Drs. Playfair and Sloan on this subject, seeing that I am almost daily witnessing the beneficial results of this treatment.

When I first began the study of the diseases of women, about fifteen years ago, I was in the habit of employing the nitrate of silver, according to the method then practised—viz., by passing the solid stick as far up the cervix as possible; but I soon found that this was a clumsy and inefficient method, and often produced a greater local effect than was desirable, more of the agent being used than was necessary. This led me to employ the mild, hard, and less soluble caustic points used by oculists. But this did not satisfy me, and I had recourse to a more accurate mode. I fused the solid nitrate in a platinum crucible, and then dipped into it a platinum probe, two inches and half long, and of the size of a No. 1 catheter. By repeated dippings I got as much as I thought necessary, and I was thus able to apply the agent to the whole length of the cervical canal, and, if necessary, to the uterine cavity, as high as the fundus. In this way I could make sure, not only of the amount of the nitrate used, but also of the extent to which it was applied, and I obtained excellent results.

This did not, however, prevent me from experimenting largely. For this purpose I had a wide field in the out-patient department of the Samaritan Free Hospital, where I had several thousands of patients pass under my hands—in one year as many as a thousand. I experimented with other agents, such as carbolic acid, iodine liniment, sulphate of zinc, styptic colloid, and a solution of sixty grains of nitrate of silver in half an ounce each of nitric acid and distilled water; but I could not bring myself to try the nitric acid alone. I found the zinc and colloid of little or no value, and the solution I reserved for special cases, which came “few and far between.”

The result of this experimentation is the practice I now adopt, which is as follows:—

When the secretion of mucus, or muco-pus, is very excessive—and in some cases as much as a small teaspoonful can be obtained from the vagina and cervical canal—and, as is usually the case, the cervical canal is very large, the calibre generally corresponding with the amount of discharge, I first apply the above solution, a few days after a menstrual period. At the end of a week the visible effects have passed off and the discharge is thinner and less abundant, and I then apply the carbolic acid (nineteen of the crystals to one of water), repeating it at intervals of a week, except when interrupted by a period, and taking care not to apply it within a week of an approaching one, until a cure is effected. In milder cases I

employ the nitrate of silver first, and, if, after a time, the carbolic acid appears insufficient, and in the convalescent stage of the more severe cases, I have recourse to it again, but only just after a period. In the earlier years of my practice I observed rather more contraction of the os and cervical canal than I considered desirable, after using the solid nitrate, and in one very severe, long-standing, and obstinate case, in which I applied my solution three or four times, but in no case to an inconvenient extent. In a few cases I still find the nitrate of silver alone sufficient, in one or two applications, but for the most part I use the carbolic acid. But I cannot say that I have obtained the wonderful results we ought to expect if Dr. Playfair be correct when he says, "practically I find that two applications, at an interval of three or four days from each other. . . . are all that is required." I cannot imagine that Dr. Playfair has in view the severe cases to which I refer, nor have I found any advantage from the more frequent applications—viz., at intervals of three or four days instead of six or seven.

But there is another method of treatment which has yielded still better results in my hands. This consists in the dilatation of the cervical canal by means of a sponge tent, as the first step. The tent must be left in for twenty-four hours. By this time, what with the compression of the swollen mucous membrane, and what with the removal of exuberant granulations to which the sponge adapts itself, and which are torn off as the sponge is withdrawn, a clean surface is obtained. A strip of lint, saturated with glycerine containing iodine liniment, in proportions varying with the circumstances of the case, is substituted for the sponge. The lint is to be renewed daily for ten or fourteen days. The iodine, however, is not to be used every day, and its subsequent applications, which should not be oftener than every third day, should be in more diluted form. At the end of the period above named the case will probably be well. Anyhow the treatment must be stopped two or three days before and during the next menstrual period. If after this anything more should be required the carbolic acid will be found the appropriate agent, and probably the "two applications" of which Dr. Playfair speaks will be all that is required. In this way cases may be cured in one or two months, instead of three to six months, or even more.

The cases in which this treatment can be employed are few and far between, as it requires that the patient should be within easy reach. Nor is it always easy to persuade a patient, who perhaps only complains of "such a discharge" with a little back-ache or bearing-down, or other indefinable and varying symptoms, to submit to enforced rest in bed for a fortnight.

The principle of the treatment is the very obvious and common-sense one, daily acted upon by surgeons in the treatment of indolent granulating wounds—viz., first the destruction of exuberant granulations, and then daily dressing. A surgeon would not dream of treating, on the surface of the body, a state of things such as the mucous membrane of a uterus affected with chronic catarrh, presents to the gynecologist, by applying something to it once a week, or even at the shorter interval of three or four days, and leaving it in the interval to take its chance. Why should not the gynecologist do likewise? But in practice it is more difficult; and in this the gynecologist is at a disadvantage.

One great advantage of this method is this—viz., that we need not fear subsequent contraction of the canal; and when the condition is a sequence of a bad “getting about” after a confinement, with subinvolution of the organ, we kill two birds with one stone—we cure the unhealthy condition of the mucous membrane and the subinvolution at the same time.

A word or two about the mode of applying liquids to the interior of the uterus. When a probe covered with cotton-wool is dipped into a liquid it is apt to take up more than is required. The consequence is, that as the probe is pushed up into the uterus the excess is squeezed out and runs over the cervix into the vagina, where it is not required. The ordinary (so-called) medicated wool is very inconvenient, because its capillary properties are interfered with by the presence of oily matter used in its preparation. Von Brun’s preparation, which is free from oil, is much to be preferred, and the quantity of liquid can be better regulated.

Dr. Playfair recommends a probe with a bulbous point. This is a mistake; or, at least, an inconvenience. Long before the publication of his lecture on this subject I had been in the habit of using a pointed (not too sharp) instrument. This form is much more convenient. I have never known the cotton-wool come off. The probe—aluminium is undoubtedly the most convenient metal—must be wrapped with the wool for two-thirds of its length, or more, and it should not be straight, but very slightly curved. Nor is it at all necessary to roughen it, as Dr. Sloan recommends. I always remove the wool with the ordinary gynecological forceps, and my difficulty is to prevent the metal getting too rough from contact with the toothed surfaces of the forceps. The trouble of getting the wool off a bulbous-pointed probe is very irritating. It is all very well in the wards of a hospital where you have a nurse to do this for you, but it is an intolerable nuisance in private practice.

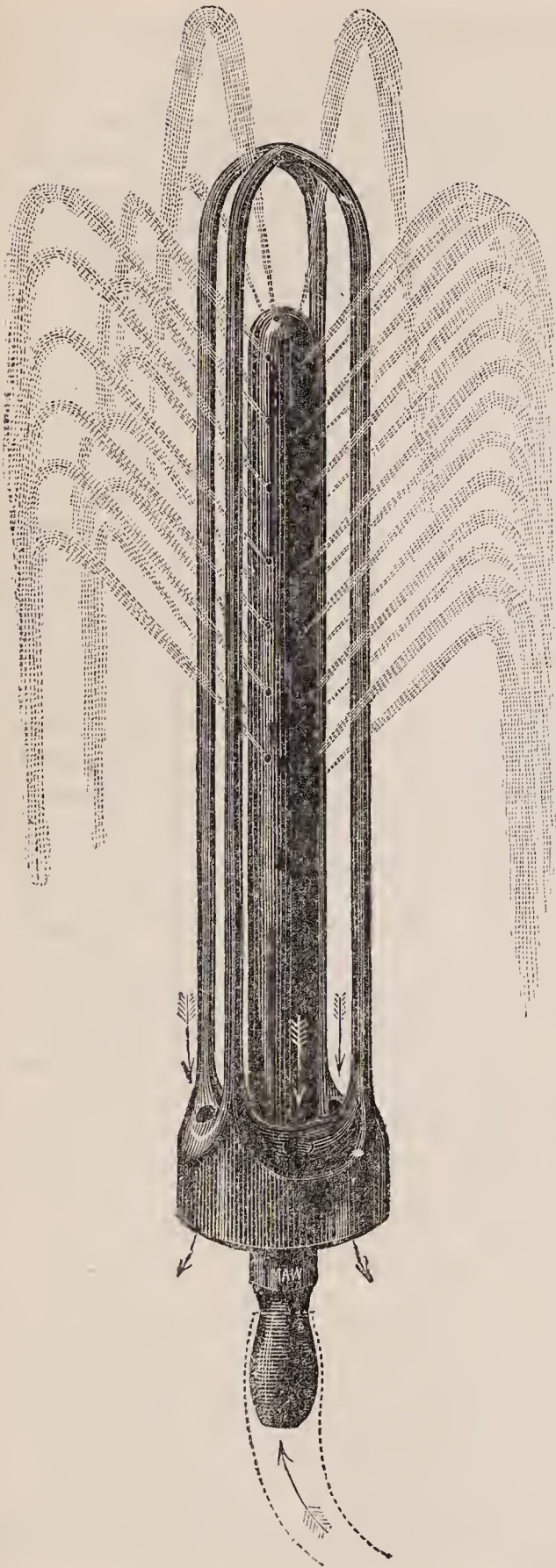
It is not within the scope of these notes to enter upon a dis-

cussion of the other conditions requiring intra-uterine medication. I have limited myself to the question of the treatment of chronic catarrh as the most frequent, and, in the experience of some, one of the most obstinate affections coming under the notice of the gynecologist, and I trust the result of this discussion will be that we shall hear less of the *hundreds* of cases in which the heroic treatment by fuming nitric acid is employed, and that intra-uterine medication will be established on a safe and scientific basis.—*Obstetrical Journal*, May 1879, p. 82.

68.—A NEW FORM OF FEMALE SYRINGE.

By HENRY GREENWAY, Esq., M.R.C.S., Plymouth.

The want of an efficient instrument for bringing various forms of injections in contact with the entire surface of the vagina and the os uteri has long been felt, and many of the leading writers on the diseases of women refer to the inadequate means generally employed for this purpose. In order to cleanse the vagina, or locally treat its diseases by injection, it must be dilated in such manner that its walls may form a tube, the dilator itself offering as little surface as possible; and the fluid to be injected should be delivered freely and from many points. These requirements can be fulfilled by the invention now brought before the notice of the medical profession. The accompanying engraving gives a very good idea of the new appliance. It consists of four small-size rods, about six inches long, curved at their upper ends so as to meet at a central point, and connected at their lower ends with the outer margin of a cylindrical stock, the spaces between the rods being equal. This part may be termed the dilator. In the centre of the stock is a hole in which is fixed a vagina-tube, which extends upwards nearly to the point of the dilator, and downwards about an inch below the stock, and it has four rows of perforations which face the spaces midway between the rods of the dilator, each perforation being directed diagonally upwards. On the lower end of this tube is fixed three or four inches of India-rubber tubing to receive a nozzle. The vagina-tube should be made of vulcanite, or some incorrodible material, but the dilator may be made either of the same material or of hard and stiff metal. The other portions of the apparatus possess no novelty, and consist of a half-gallon tin reservoir, having an outlet in the bottom leading into a nozzle soldered on the outside. On this nozzle is fastened one end of an India-rubber tube, eight feet in length, the other end being fixed on one of the nozzles of a union tap. By connecting the other nozzle of the tap with the vagina-tube the apparatus is ready for use.



Directions for use.—Close the tap and place the injection in the reservoir, and hang it from a bed-post or a wall about seven or eight feet from the floor. The patient should then introduce the dilator and tube into the vagina as far as possible, and sit on a bidet or other vessel raised to a convenient height. By turning the tap the injection will flow freely, and, whilst flowing, the instrument should be moved to and fro, and occasionally slightly rotated. If only a small medicated injection be required, it may be forced into the vagina-tube from an India-rubber bottle furnished with a bone nozzle, instead of using the tin reservoir; but it is advisable to first cleanse the vagina with water if there be much morbid secretion. The vagina-tube and dilator may also be used in combination with any suitable form of injecting instrument. It will thus be seen that when fluid is injected into the vagina by this means, the jets are directed diagonally upwards, and, striking its walls, rebound in splashes to the opposite side of the passage (the perforated tube being surrounded by a free space), and the injection escapes from the vagina through holes in

the stock. The arrows in the engraving show the course of the currents.

The dilators are of three sizes—large, medium, and small—and the whole apparatus is manufactured by Messrs. Maw, Son, and Thompson, of Aldersgate Street, London, who have satisfactorily carried out every detailed instruction. This invention, which has been patented in France, may, when made in suitable sizes, be used with advantage in any passage or wound requiring dilatation during irrigation.—*Lancet*, May 10, 1879, p. 662.

69.—ON THE TREATMENT OF UTERINE TUMOURS BY DILATATION AND THE ECRASEUR.

By Dr. GEORGE H. KIDD, Master of Coombe Lying-In Hospital, Dublin.

We have recently had a new kind of dilating material made known to us under the name of tupelo-tents that may, at the second sitting, be advantageously used instead of sea-tangle. This substance has been brought into notice by Dr. Sussdorff, of New York, in a paper published in the New York Medical Record of July 1877. The tents are formed from the root of the *Nyssa aquatica*, which grows in the swamps of the Southern States of America. As imported into this country, they are too short to be of much use for dilating the uterus; but Messrs. Fannin and Co., of Dublin, have procured them for me of the full length required. These tents swell more quickly, and in proportion to their size when dry to a greater degree, than does the sea-tangle; but the tangle can be more easily introduced in the first instance, and, from its slower and more gradual action, will probably be found less painful and safer for the patient than the other. As soon, however, as the process of dilatation has commenced, and the tissues have become softened and relaxed, the tupelo will complete it more quickly and thoroughly than the sea-tangle. If three tupelo-tents can be introduced at the second sitting, and along with them four or five pieces of No. 6 sea tangle, the uterus will generally be found sufficiently dilated at the end of a further twenty-four hours to permit the removal of a tumour measuring from three to four inches in diameter.

The dilatation of narrow passages dates from the earliest ages of surgery, prepared sponge being the substance generally used for the purpose; but, till suggested by Sir James Simpson about thirty years ago, the exploration of the uterus by its means had not been attempted. Till then, as Sir James has stated, intra-uterine polypi “were generally considered as placed beyond the pale of any certain means of detection, or any possible means of operative removal.” But now, following in his footsteps,

and using the improved methods at our disposal, large tumours, such as even Sir James Simpson would not have thought of touching, have been made accessible, and brought within the domain of surgery. The dangers and inconveniences, however, attendant on the use of the sponge, have deterred many from attempting to dilate the uterus at all, or have led them to do it timidly and inefficiently; thus Dr. Emmett, in his recently published book, a work which would amply prove him, if we did not already know it, to be not only a bold but a most skilful and successful surgeon, though he describes a modification of the sponge-tent, and a special instrument for dilating the uterus, seems to scarcely use either for purposes of treatment, but for diagnosis only; and, indeed, specially recommends, in speaking of large tumours, that no attempt should be made for their removal till they appear at the os and begin to come down into the vagina. But we all know that, in the majority of cases, a woman's health is shattered and her life often placed in extreme jeopardy long before the tumour makes its appearance at the os, or begins to press on it. As a further example, I may mention that one of the specimens on the table was removed from the uterus of a lady who for some time was under the care of one of the most eminent gynecologists and successful operators of the age, who, after spending a week in trying to dilate with sponge-tents, gave up the attempt, and recommended that the uterus should be extirpated, or the ovaries removed by Battey's operation; yet, after the use of two series of sea-tangles for forty-eight hours, the tumour, which was imbedded in the posterior wall of the uterus near the fundus, was safely removed by a combined process of enucleation and avulsion: an operation hazardous enough, but certainly much less so than the extirpation of either uterus or ovaries. In another case, which occurred about two years ago, the patient had been assured by one of the leading gynecologists in the north of England that the tumour, the nature of which he had fully recognised, could not be removed by any possible means, yet, by the means now detailed, it was, in a space of forty-eight hours, brought within reach and removed: and the lady, who had lived several years in sterile marriage, has since given birth to a child. I have not the tumour here to exhibit, for she insisted on taking it home with her to show to her friends that such tumours could be removed.

Having dilated the uterus and made a tumour accessible, the next step is to remove it. In the paper on uterine polypi already alluded to, the mode of removing a polypus with an *écraseur* is described, and illustrated by a diagram; even large tumours, if prominent into the uterine cavity, may be removed in the same way. The uterus is first drawn down to the vulva, having been

seized by a strong vulsellum; then the tumour is laid hold of either with a fine vulsellum or tenaculum, or with the "spiral instrument" described and figured in his book by Dr. McClin-tock, which is, indeed, nothing more or less than a long corkscrew, and the loop of a wire *écraseur* is passed round its base. In my first paper, I recommended that this should be a soft iron wire; but I now find that, for large tumours, a finely tempered steel wire is the best, such as a piano-string, as it, though it may be compressed in passing through the os, opens again by its own elasticity when it gets into the cavity of the uterus, and is, therefore, more easily passed over the tumour and it is, besides, firmer and stronger than the iron, and will bear a greater strain. In using an *écraseur*, one of two effects will be produced. If both ends of the wire be attached to the screw, then a purely crushing movement is produced. When the screw is worked, the wire constricts the tissues till it gradually crushes its way through. If one end of the wire be attached to the screw and the other fixed, then a cutting motion is obtained combined with the crushing. This combination of cutting and crushing enables us to divide tumours that would resist and break the strongest crushing instruments; but to obtain the combined action of cutting and crushing, the screw holding the wire must travel double the distance required in the crushing movement. With the ordinary *écraseur*, consequently, it is often necessary to stop in the middle of the operation, and readjust the wire before the operation can be completed. This might, perhaps, be obviated by using Weiss's *écraseur* which has a windlass to wind up the wire, but the instrument is very cumbrous, heavy, and inconvenient, and I believe it has never come into use. A Dublin student, Dr. Denham, son of Mr. Denham, Ex-Master of the Rotunda Hospital, has, however, invented a simple instrument by which either a crushing or a combined crushing and cutting action can be obtained; and by its use, what has hitherto been one of the greatest practical difficulties in cutting thorough the base of large sessile tumours will probably be quite overcome. The difficulty consisted in this, that, to encircle a tumour of, let us say, from three to four inches in diameter, the loop of wire must be more than from nine to twelve inches in length, and if only one end of it be attached to the screw so as to give the combined cutting and crushing movement, the *écraseur* must be so long as to be unwieldy in its proportions and weakened in its powers. Denham obviates the difficulty by making one end of the wire traverse the whole length of the screw, and enabling us, this being accomplished, to make the other end, by a very simple movement, take up the action and follow the same course. An inspection of the instrument which lies on the table will show at a glance how this is accomplished.

What has been said so far, as to the removal of the tumours after access to them has been obtained by dilating the uterus, refers to intra-uterine tumours—that is, those which have grown into the cavity of the uterus; but interstitial tumours, or those imbedded in the substance of the uterine wall, when they approach closely to the mucous membrane, often give rise to hemorrhage, as serious and as injurious to life and health as that caused by intra-uterine tumours. The avulsion or enucleation of such tumours has long been practised; but, till Dr. Marion Sims and Dr. Gaillard Thomas described their mode of operating and devised instruments for the purpose, it seemed to me too dangerous to be attempted, except in extreme cases. Such tumours can now, however, be removed almost as safely as those which have grown into the uterine cavity; but, when they lie high up in the cavity of the uterus, full dilatation must first be effected, and for this purpose the method now described appears to me to be the safest and most efficient.

A series of observations on the shape of the uterus, when enlarged by the growth of a tumour in its cavity or in its walls, has induced me to suggest a few simple rules for the diagnosis of the relations and position of the tumour, which seem likely to enable us to know, before proceeding to dilate, the conditions that will probably be met with. The rules may be summed up as follows. When we have evidence of the existence of a tumour, and the cavity of the uterus is enlarged, if the uterus be uniform in shape, without any bulging out or unequal enlargement of any of its walls, the tumour will probably be found to be more or less pedunculated, growing from the fundus of the uterus and hanging down into its cavity. If the uterus be found unequal in its outline, bulged out at one side and straight at the other, and if, on introducing the sound, it pass along the convex or bulged-out side, then the tumour will be found to be growing from the wall opposite to where the bulging-out occurs, and projecting into the cavity. If this bulging-out be sudden and much marked, the tumour will probably be pedunculated; if the bulge be less marked and gradual, the tumour will probably be sessile, and projecting into the cavity from the wall opposite to the bulge, and may be so far interstitial as to have a thin layer of muscular fibre covering it over under the mucous membrane. If the uterus be bulged out in the same manner at one side, and the sound pass along the straight instead of the convex or bulged side, then the tumour will be found to be interstitial, and deeply seated in the uterine wall, closer probably to the peritoneal than the mucous surface. If further experience should confirm these rules they will, I hope, afford us some aid towards deciding in what cases an operation

should be urged, and in what it should be undertaken with more caution.—*British Medical Journal*, August 16, 1879, p. 241.

70.—ON ANTISEPTIC OVARIOTOMY.

By J. KNOWSLEY THORNTON, Esq., M.B., C.M., Surgeon to the Samaritan Free Hospital for Women and Children.

I believe we are now able to say we can perform ovariectomy with perfect antiseptic precautions, in any case in which there has been no previous tapping, or in which tapping has been performed antiseptically. I now always tap with the spray, and as carefully as I perform any other surgical operation antiseptically, and since doing so I have never seen any cyst inflammation, as it was called; it should be called admission of the causes of putrefaction into a cyst by tapping. In cases which have been tapped before they come to me, I never feel certain that I can perform aseptic ovariectomy, because the cyst may already contain the causes of putrefaction; and if this be so in a difficult case, with adhesions, we cannot be certain that some of the putrid material will not pass over portions of the wound or peritoneum without being rendered innocuous. The mere fact that the fluid is *not* offensive is no guarantee that it contains no causes of putrefaction, for I have found fluid full of active bacteria, yet without odour; moreover, when the spray is playing over everything, it is difficult to detect odour unless it be very powerful, and one cannot pause in an ovariectomy to examine the fluid microscopically. I have observed many facts which make it probable that the causes of putrefaction may be present in a cyst without the patient presenting the usual symptoms of cyst putridity, and without odour from the cyst contents. And yet, when exposed to the air, and brought in contact with fresh blood, serum, and wounded tissues, these latent causes of putrefaction become active. They are, in fact, dormant in the cyst. This is too difficult a subject to discuss in this paper, and I want more facts; but it is necessary I should mention it.

One word as to tapping. I believe it is not sufficiently recognised in the profession that this proceeding is one attended with considerable risk, especially in inexperienced hands. Antiseptics carefully employed remove, as I have said, the risk of so-called cyst inflammation, which is by no means an uncommon result without their use. Two other risks remain—1. The danger of hemorrhage into the cyst from puncture of a vessel in its wall. 2. The danger of infection of the peritoneum by escape of some of the contents of a papilloma-bearing cyst into the peritoneum, either at the time of tapping, or afterwards from the puncture in the cyst wall remaining patent

after the external opening has closed. Both these, I know, are very real dangers, even in the most experienced hands. The operation of tapping a cyst has always seemed to me a most unscientific one, for we puncture a highly vascular structure in the dark, and with no certainty of being able to avoid large vessels, however great our care, and if the cyst be non-adherent it may be quite impossible to secure a bleeding vessel without an incision through the parietes. I have seen ample evidence when performing ovariectomy after tapping that such hemorrhages do occur, and I know that they are occasionally fatal. So long as ovariectomy was a very fatal operation, and tapping a *comparatively* safe one, it was necessary to tap. I now never advise the operation unless it is absolutely necessary to perfect diagnosis, or in cases of such great distention that it is well to relieve the heart, lungs, and kidneys a few days before performing ovariectomy. I am convinced that tapping should bear a much larger share of the general mortality following ovariectomy than it does at present. I have not lost a single uncomplicated case since I began antiseptic ovariectomy, and I believe a simple antiseptic ovariectomy is a safer operation than a cyst-tapping, and I am much inclined to think an antiseptic exploratory incision is a safer operation than an exploratory tapping. The imperfect aid to diagnosis given by the latter can afford no aid comparable to the perfect knowledge we obtain from the former.—*Lancet*, Sept. 20, 1879, p. 418.

71.—THE DISCUSSION ON THE USE OF THE FORCEPS.

By the EDITOR OF THE BRITISH MEDICAL JOURNAL.

The discussion on the use of the forceps and its alternatives in lingering labour at the Obstetrical Society of London has been a valuable one and will bear fruit. The general tendency on the part of the speakers, more especially on the part of those who have had the largest share of difficult midwifery, was to prefer the use of the forceps to ergot, whenever a choice lay between these two; and the extended use of the forceps as the best means of lessening resort to craniotomy; for example, the application of the long forceps to the head above a contracted brim, instead of perforation of the child's head or turning, as was the practice before the era of the long forceps. Indeed, nothing has ever been brought to light more clearly in any discussion, than has the fact that, in proportion as the long forceps has been improved by lengthening it and by adding the pelvic curve, so has its accompanying use supplanted or eliminated ergot, turning, and craniotomy. The result of this has been a constantly diminishing ratio of maternal and foetal mortality.

Another interesting point which has been brought out and emphasised in the discussion is the fact that, for the growth and development of the forceps, the murky atmosphere of the large manufacturing and densely populated towns, such as London, with its four million inhabitants, Manchester, Birmingham, and Dublin, has been more beneficial than the clearer atmosphere of sparsely populated districts, like Scotland. Indeed, it is only where large masses of human beings are crowded together under unfavourable hygienic and other conditions, that a large proportion of difficult labours from pelvic deformity and other causes is found. And so it is that the first long forceps appeared in England, and that to-day, when the long forceps is almost universally used in the large towns of this country and in Dublin, the short double-curved forceps of Simpson is still the most widely used in Scotland.

On the other hand, it seems fairly agreed upon, that the practice recommended by Dr. George Johnston of applying the forceps to the head above the brim before the os is dilated, and sometimes before even the membranes are ruptured, cannot be supported by the mass of obstetricians at large. Most of the speakers, and among them Dr. Grailly Hewitt, endorsed the fourth proposition laid down by Dr. Barnes in his masterly address, that "in proportion as the head is arrested high in the pelvis, in the brim, or above the brim, the necessity, utility, and safety of the forceps become less frequent." Dr. Grailly Hewitt admitted that the cases in which it is necessary to apply the forceps to the head above the brim do occur, but they are few. As regards Dr. Barnes's first proposition, that "in lingering labour, when the head is in the pelvic cavity, the forceps is better than its alternatives," the consensus of opinion was, as might have been expected, almost universal. It was generally agreed that, in such a case, the termination of labour by the application of the forceps was not only a simple and easy operation for the practitioner, but a humane and beneficial procedure for the mother and the child. In other words, the "low operation"—or as Dr. Barnes more precisely puts it, the intrapelvic operation—is now recognised as a safe and justifiable proceeding in all cases of lingering labour. Its chief alternative, ergot, has for some been dying a natural death; and it was with considerable satisfaction that we heard Dr. Lombe Atthill state, in the course of the discussion, that its use is abandoned and even prohibited in the Rotunda Hospital. There is nothing that ergot can do that cannot be done more safely and effectively by the forceps. The use of ergot in the arrest of hemorrhage is of course outside the discussion; no one denies its utility in this respect. The good results attending the substitution of the forceps for ergot

are strikingly set forth in the experience of Mr. Alderson. When this gentleman began practice, he never, or at all events hardly ever, used the forceps at all, but he used ergot frequently. During this period, he used to have an enormous number of still-births. Gradually he cultivated the use of the forceps; and, as a natural result, his still-births diminished, until now, when he applies the forceps in all those cases where formerly he would have administered ergot, he has no still-births at all. Similar results to his have been observed in comparing the numbers of the still-births inside lying-in hospitals where the timely use of the forceps is secured, and those outside, where the midwives do not send for assistance, but give ergot.

The discussion has, we believe, given the *coup de grace* to the antiquated idea, prevalent in the days of Dr. Robert Lee, who performed craniotomy in one hundred and eighty-six cases and used the forceps in only fifty-three cases, that the forceps is a dangerous instrument. No instrument in the whole range of medicine has ever saved more lives and more human suffering than the forceps, the invention and perfection of which constitute one of the chief glories of English midwifery.—*British Medical Journal*, July 19, 1879, p. 95.

72.—ON THE EARLY USE OF THE FORCEPS.

By the EDITOR OF THE MEDICAL TIMES AND GAZETTE.

We propose to comment on the use of forceps when the head is high up, or the os uteri not completely dilated. It is this practice with which Dr. George Johnston's name is associated. He has advised it, has practised it upon a large scale, and has published the results of his practice. Dr. Johnston did not in all his cases apply forceps before full dilatation of the os uteri. His figures embrace also cases of the slighter degree of difficulty and risk to which Dr. Playfair's advocacy of the frequent use of the instrument is restricted. This inclusion of cases of different kinds prevents us from taking the results obtained as representing the risk incurred by using forceps when the head is on the perineum. Yet, in so far as the figures overstate the mortality of the low operation, it is manifest that they understate that of the high one.

Dr. Johnston's statistics form one of the most valuable contributions ever made to the literature of the forceps. It is not merely because their number—7862 cases, extending over a period of seven years—is large enough to prevent mere temporary or accidental causes from influencing the general result: they have an especial value, because the cases from which they are derived occurred in an institution (the Rotunda Hospital,

Dublin) the statistics of which have been published for a series of many years, and the practice and arrangements of which have been supervised by obstetricians of great eminence. Now, it happens that from 1826 to 1833 Dr. Collins was Master of the Rotunda Hospital, and this great obstetrician used forceps very rarely—once in 608 deliveries. Dr. Johnston used them once in ten. Therefore we here have the two extremes of practice with relation to the use of forceps.

The statistics of these two Masters of the Rotunda Hospital seem to be exactly comparable—the same hospital, patients of the same nation and the same class. Such differences as there were, all appear in favour of Dr. Johnston's patients. From 1826 to 1833 Dr. Collins had in hospital 16,414 cases; while from 1868 to 1875 Dr. Johnston had only 7862. It is therefore clear that, the hospital remaining of the same size, and assuming that under the later Master the number of beds in each ward was a fair and proper complement, the hospital must in Dr. Collins's time have been much overcrowded. This inference is supported by the fact that under Dr. Collins's mastership there was an outbreak of puerperal fever. No such thing occurred under Dr. Johnston's management. Surely, also, some lessons have been learnt in sanitation and in the treatment of disease during the thirty-five years which separated Johnston from Collins. In these points, then—the absence of overcrowding, freedom from the puerperal fever contagion, and enlarged medical and sanitary knowledge—the advantages which Dr. Johnston's patients enjoyed might have been expected to make his mortality lower. But the conditions were the same in every other respect but this: that Dr. Collins left his cases largely to nature, using forceps only once in 608 times, while Dr. Johnston interfered frequently—once in ten times—and early; often before the os uteri was fully dilated.

The only way to judge these different kinds of practice is to compare the results of the whole number of cases. It is misleading to compare forceps cases with forceps cases. This was pointed out by Dr. Kidd in the discussion which took place in Dublin upon Dr. Johnston's report. The less the need for the operation, the simpler it becomes. One who restricts the operation to really difficult cases will of course have a larger proportional mortality, in his few forceps operations, than one who uses the instrument in a great number of simple cases. Dr. Johnston says, "Our established rule is, that so long as nature is able to effect its purpose without prejudice to the constitution of the patient, danger to the soft parts or the life of the child, we are in duty bound to allow the course of labour to proceed. But as soon as we find the natural efforts are beginning to fail, and after having tried the milder means

for relaxing the parts or stimulating the uterus to increased action, and the desired effects not being produced, we consider we are justified in adopting prompter measures, and by our timely assistance relieve the sufferer from her distress and danger, and her offspring from an imminent death. . . . The more we consider the benefits arising from timely interference, and the good results which follow it, the more we are induced to pursue the system we have adopted." We quote this to point out that it begs the question at issue. It has long been agreed that if mother and child are in imminent peril it is proper to interfere. What needs proof is, that foetal and maternal life really are so often in danger as in Dr. Johnston's practice it is assumed that they were. The question is not whether, in really difficult cases, the early use of forceps gives a better result than the late resort to them; but whether, in 10 per cent. of labours, the early use of forceps is better than letting the case alone.

We look to the Rotunda Hospital reports to see the result of the so-called "timely" interference. The figures answer with appalling clearness. The result of the practice has been to *more than double* the maternal mortality. At the rate of mortality which followed Collins's practice (1 in 100), Johnston should have had 78 deaths. He lost 169 patients—91 in excess of what might have been expected had the cases been treated on the principles of Collins. One patient out of every 47 died. Taking into consideration all the concurrent advantages which Johnston's patients enjoyed, this number is so large as to be terribly free from ambiguity. This death-rate demonstrates, in a manner which we hope it may never be necessary to repeat, the danger which the mother runs from the uncalled-for use of forceps.

But the practice is said to be partly justified by the number of foetal lives which it saves. It is difficult to speak with certainty about this, because Dr. Johnston has not made known the total foetal mortality of his practice. But Dr. Barnes, calculating the whole from a part, puts it at 6·1 per cent. The total foetal mortality of Collins's practice was 6·8 per cent. But Johnston does not include, in his statistics, all his premature deliveries, while Collins does. If we take out from the figures of Collins all those born at or below six months, his foetal mortality is brought down to 6·2 per cent.; and if we exclude from it all those born before full term, it is lowered to 5·1 per cent.—a ratio below that of Johnston. And this is not all. It will occur to every obstetrician that one who used the forceps so seldom as Collins did probably, more often perforated. And such was the fact. Collins perforated 118 times, or once in every 141 cases. Johnston resorted to it only 28 times, or once in every

281 cases. It may, therefore, be presumed that some of the children destroyed by Collins might have been saved by a proper use of forceps, or by turning, and thus his foetal mortality been reduced still further below that of Johnston. The conclusion follows, that the frequent use of forceps by Dr. Johnston slightly increased the foetal mortality.

There is one other result which demands attention. It is the effect of forceps upon the child after delivery. Of the children delivered alive by forceps under Johnston, one out of every 11·5 died soon after birth. Of those born alive under Collins's management, only one out of every 58½ died before the mother left the hospital. The difference is great, and, we think, important. This part of the subject is one which needs careful consideration.

Dr. George Johnston deserves the greatest praise and the warm thanks of the profession for having so fully and so boldly published the result of the great "experiment" (as Dr. Roper termed it) which he has carried out in the Rotunda Hospital. His figures show, beyond doubt, that the effect of the too frequent use of the forceps is to largely increase maternal mortality; and such effect as it has upon foetal mortality is to slightly increase that also. The only thing we note with some surprise is, that Dr. Johnston does not seem to see what his own figures prove.

There are other points to which we might advert, such as the state of the perineum after forceps; but space forbids us. We cannot conclude without expressing our sense of the importance and value of the speech of Dr. Roper, in which he subjected the practice of Dr. Johnston to perhaps the most searching criticism it has yet received. The figures we have quoted deserve to be carefully pondered by every practitioner of midwifery. We hope the published results of Dr. Johnston may tend to check any tendency to over-meddlesomeness. We have the broad fact before us, resting on an experience of seven years, that too frequent interference with forceps has more than doubled maternal mortality, and probably increased foetal mortality. To have set this in relief is perhaps the best result of the recent discussion. It must also be remembered that, in comparing Dr. Johnston's maternal mortality with that of Dr. Collins, we are taking for comparison a series of cases in which probably the forceps was used too seldom, and the perforator too often, and the mortality of which was moreover raised by an outbreak of puerperal fever, and is in fact above the average childbed mortality. If we were able to take for comparison a set of cases treated in the best manner, and under the most favourable conditions, Dr. Johnston's figures would show to still greater disadvantage.—*Medical Times and Gazette*, August 2, 1879, p. 126.

73.—DR. GOELET'S SELF-RETAINING SIMS' SPECULUM.

The Sims' speculum is, undoubtedly, a most invaluable instrument, but its greatest drawback is, that it requires an assistant in using it. If the practitioner attempts to use it alone he occupies the left hand with holding the instrument in position, while the right is engaged in elevating the anterior wall of the vagina by means of the depressor; thus, both hands being in use, manipulation is impossible.

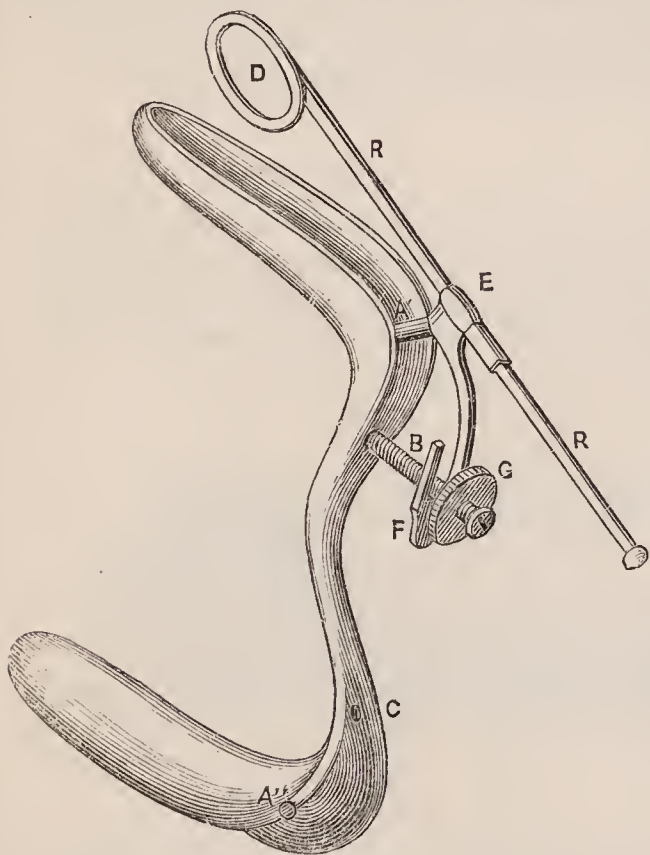
The present modification, designed by Dr. Goelet, of New York, consists of a movable attachment of a depressor to an ordinary Sims' speculum, and will remedy this disadvantage.

Dr. Thomas' modification of Sims' speculum does the same thing in a measure, but this also has its disadvantages. It consists of one blade fastened to a handle, with the depressor attached to a lateral arm projecting from the base of the blade, which is much widened at that point to accommodate it. The attachment is fixed, *i.e.*, cannot be removed, and there is only one size blade.

The advantages then of the present modification are, *viz.*: The attachment may be made to any Sims' speculum. It may be used without an assistant. The attachment, which is movable, may be fixed and used with either the large or small blade.

The attachment being movable, the speculum may be used without it if so desired. The speculum may be first introduced and the depressor attached afterwards. When introduced and the depressor fixed in the desired position by the thumb-screw, it will retain this position without further support. This is a point of gain which had not been anticipated for it, but which has been proved by repeated experiments.

The attachment consists of a small arm *only* large enough to hold a screw, rivetted to the



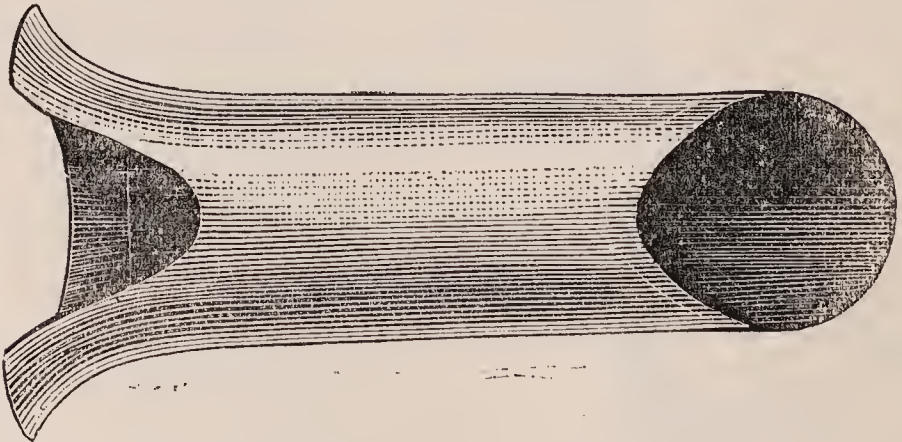
[We are indebted to the courtesy of the Editor of the Medical Press and Circular for the use of the woodcut of this Speculum.—EDS.]

Sims' Speculum at A' where the depressor is attached by means of a lock like that of a Hodge obstetric forceps. At B a standard, upon which the thumb-screw G works, is made to screw in, and may be removed or screwed in at C when it is desired to use the smaller blade. At A'' is the arm for the attachment of the depressor to the smaller blade. The depressor is a light steel rod R R which terminates in an oval ring D, and slides up and down through the upper end of the lever at E. The lower end of the lever F slides up and down on the standard B, and may be fixed at any point by means of the thumb-screw G.

The instrument may be introduced in two ways, viz.: First, with the depressor attached but drawn back to its limit, when it will not interfere with introduction; or second, the lever and depressor are first detached, leaving only the small arm A and its screw, and the standard B attached to the Sims' speculum. The blade is then introduced in the usual manner, and carried well behind the cervix. Being steadied in that position by the left hand, the lever, with the depressor well drawn back, is attached at A with the right, and by means of the screw locked. The depressor is then slid along in the groove of the Sims' blade until it comes to its extremity, when it is separated from it $\frac{3}{4}$ of an inch. Then by placing the thumb of the left hand on the lower part of the lever at F, the cervix may be lifted into any position desired, and held there by means of the thumb-screw G.—*Medical Press and Circular*, July 16, 1879, p. 55.

74.—NEW "TOUCH AND SIGHT" SPECULUM VAGINA.

In appearance and construction the above-named speculum, manufactured for Dr. Hickinbotham of Birmingham, by Messrs. Salt and Son, of that town, resembles the ordinary Fergusson's speculum, the improvement claimed for it being that it is shorter



(so that a short finger may find it available), its outward extremity being more trumpet-shaped, and deeply notched on

the side corresponding to the lower part of the bevel, at its further extremity, so as to enable the finger to reach and examine the os without difficulty, a great desideratum in obstetric practice. It is made either in electro-plate or after the manner of the well recognized "Fergusson's Speculum."—*Medical Times and Gazette*, August 23, 1879.

75.—FLEXIBLE METALLIC FEMALE CATHETER.

The Catheter represented by the sketch on the next page, will be found very useful, combining as it does, the flexibility of the gum elastic catheter with the durability of the silver one; the surface is perfectly smooth, and the size convenient for the ordinary pocket case. The catheter is made of sterling silver, and gilt: it has a slide, to increase the length when necessary, and a syphon to cut off the flow. The idea was suggested by a prostatic catheter made on the same plan.

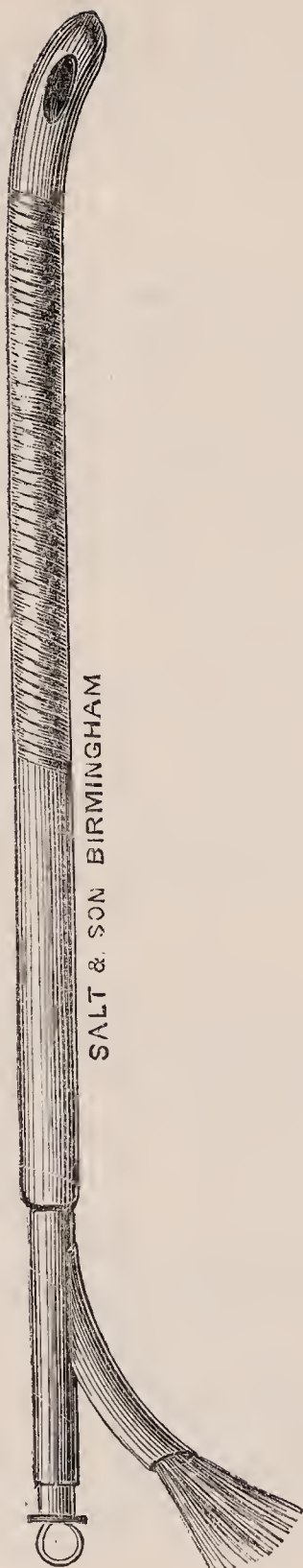
76.—AN IMPROVED UTEROMETER.

The instrument of which a drawing is given on the next page, is an improvement upon one invented by Dr. Alexander Duke, of Dublin, which was noticed and figured in the *Medical Press and Circular* of July 16. Dr. Duke says:—"The plan of measuring the depth of the uterine cavity by the introduction of the sound, and the subsequent insertion of the finger nail of the operator into one of the notches marked in that instrument has been, to say the least, uncertain in its results and decidedly inconvenient.

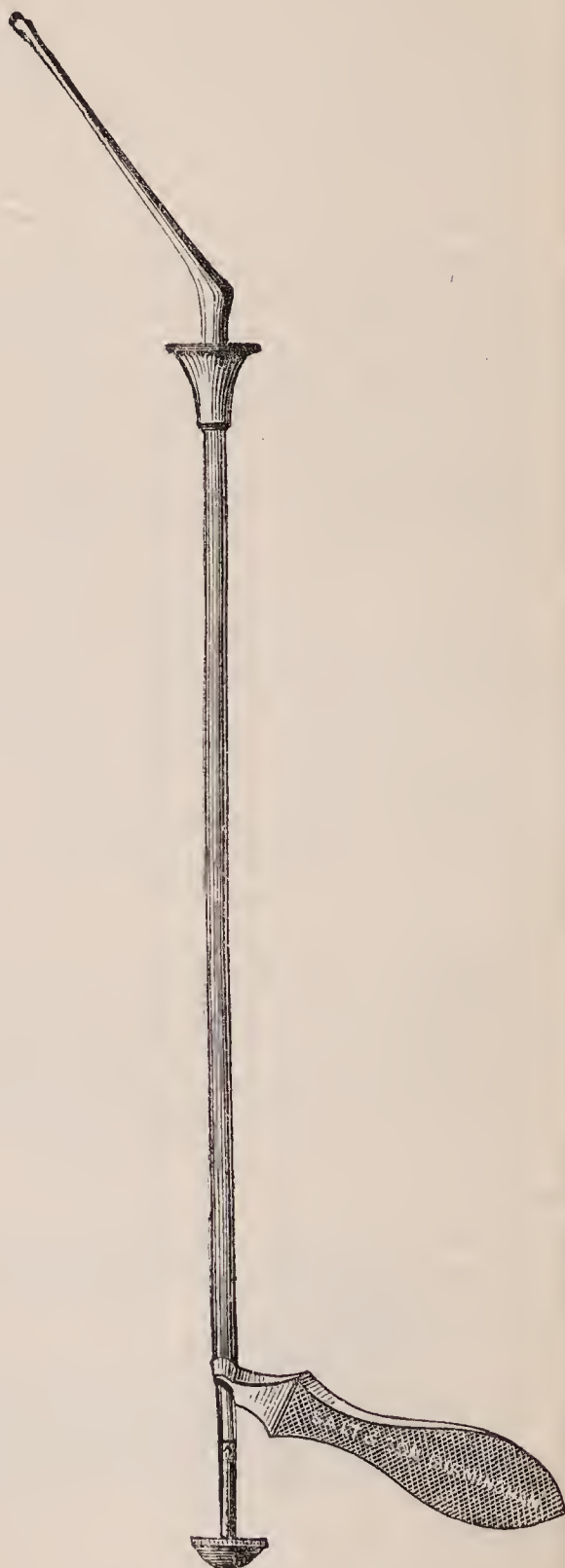
"The uterotome consists of an ordinary uterine sound (without the notches), but with sliding scale attached, graduated in inches and half inches, which can be read off exterior to the vagina, and can be used either with or without the speculum.

"The point being introduced in the ordinary way into the uterine cavity any deviation from its normal depth will (unless the os be exceedingly patulous) have the effect of pushing down the measuring scale, and thus registering the result outside; or, if the operator prefer, he can draw down the scale before introduction, and when the sound has penetrated to the fundus, slide the scale upwards till it meets the os, the projecting portion will, as by the former plan, give a similar result." (p. 41.)

The object of both instruments is the same, but the improvement consists in the sliding bar which is attached to the cushion being made to work *inside* instead of *outside* the instrument, thus diminishing its bulk, and rendering it more facile in use. The shield is set at the normal length of the uterus, any deviation from which is shown by an index on the proximate extremity. The manufacturers of this instrument are Messrs. Salt and Son, of Birmingham.



Salt & Son's
Flexible Metallic
Female Catheter.
(Art. 75.)



An Improved
Uterometer.
(Art. 76.)

ADDENDA.

77.--ON THE EFFECTS OF CHLOROFORM, ETHIDENE, AND ETHER, ON BLOOD PRESSURE.

(3rd Report on Anæsthetics to the British Medical Association.)

By Dr. JOSEPH COATS, WILLIAM RAMSAY, Ph.D., and Dr.
JOHN G. MCKENDRICK, of Glasgow.

In the report of a Committee of the Royal Medical and Chirurgical Society, to inquire into the uses and effects of chloroform, and published in the Transactions for 1874, there is a very excellent but brief record of the blood-pressure under chloroform and ether. The instrument used in these experiments was the hæmadynamometer of Poiseuille, which consists of a U-shaped tube, with mercury in the bend. One limb of the tube was connected with the femoral artery, and the rise of the mercury in the opposite limb indicated the blood-pressure. By means of this instrument, the Committee were enabled to report that, on administering chloroform, there was at first a transient rise of the blood pressure; after which, there was a gradual, but not a regular, fall. They also noticed that, when the force of the heart was reduced by chloroform to the full extent, the respiration of fresh air was at once followed by a rise of the mercury. In regard to ether, it was found that the primary rise in pressure was greater and more constant than with chloroform, and that the depressing effect was very slight or altogether absent.

In our experiments, we have been able to amplify these results; and, by means of more delicate instruments, to obtain more exact records. We have used a very complete kymograph, in the Physiological Laboratory of the University of Glasgow, made by Rudolph Rothe of Prague, by means of which the variations in the column of mercury, produced by the pulsations in an artery, are written (by means of a stem which floats on the mercury) on a sheet of blackened paper, which is carried round by a clockwork arrangement. A sheet of paper, eight feet long, is adapted to the machine; and, as three or four lines of tracing can be taken at different levels on the same sheet, a continuous tracing may be obtained of twenty-four, thirty-two, or even more feet. But, further, the sheets can be changed in a few seconds, and so it is possible to take tracings of almost any length. The instrument has also appliances by which time

could be recorded in seconds, half-seconds, or otherwise, immediately beneath the tracing of the blood-pressure. Lastly, there are two arrangements by which the exact time of administering and discontinuing an anæsthetic can be marked. In this way we have obtained records of experiments on rabbits and dogs, those on dogs being of much the greater value.

In the case of the rabbit, there is one fact of considerable interest which seems to be deducible from these records. When the animal is not fully under chloroform, any fresh administration causes most remarkable variations in blood-pressure, with retardation of the heart's contractions. There is frequently a sudden dip in the pressure to the extent of forty *millimètres* out of a total of one hundred and ten *millimètres*; and in the next few contractions, there is a very rapid rise up to the former level, to be succeeded by another sudden dip. This occurrence followed so uniformly in certain stages of chloroform-narcosis, on every approximation of a sponge containing the agent, even for a few seconds, to the animal's muzzle, that it was regarded as probably reflex. This is rendered all the more probable by the known fact that ammonia-vapour, applied to the nostrils of a rabbit, causes stoppage of the heart's action. When the chloroform is given continuously, these variations gradually cease, and there succeeds a regular and gradual fall of pressure down to zero, if the agent be pushed. It was noticed that, in one or two instances, ethidene produced sudden variations in pressure similar to those of chloroform, but that ether did not. Our experiments were not fully prosecuted on rabbits in regard to the more permanent effects of these two agents; but it may be said in general that ether seemed to have no effect on blood-pressure, while ethidene reduced it to a considerable extent, but not to total extinction, like chloroform.

Turning to our experiments on dogs, the very first observation made was an exceedingly striking one. The animal used was a black retriever, six or seven months old. Chloroform was given, and during deep anæsthesia a cannula was introduced into the carotid artery, and connected with the kymograph. By the time connection with the kymograph was established, no chloroform had been given for about two minutes. On first making the connection, the pressure registered 104 *millimètres*, which may be regarded as nearly normal; but now, without any fresh dose, the pressure rapidly fell to zero, with a remarkable retardation of the heart. Each pulsation had a height of 9 *millimètres*, and a duration of a second and a half. After this, the pressure rose with remarkable variations, equal to from 13 to 16 *millimètres*, referable to the respiratory movements. It is to be remembered that the animal had at this time to a great extent recovered from chloroform, as evidenced by the high initial

pressure; and this sudden fall of pressure, is apparently reflex in character; the heart being, perhaps, more liable to such influences under the conditions present. A fresh administration of chloroform at this time led to a fresh variation of pressure somewhat resembling those already referred to in the case of rabbits. On continuing the administration, all irregular variations were abolished, and the pressure gradually fell; but the agent was not in this instance pushed very far. During recovery from this administration, seventy seconds after chloroform had been removed, and when the pressure had risen to 66 *millimètres*, there was, without any apparent cause, an occurrence somewhat like that at the outset, but less in degree. The pressure fell to 20 *millimètres*; and the heart's pulsations became unfrequent, each pulsation taking a second and a half. This continued for six beats, when the pressure rose slowly to 100 *millimètres*, with recovery of the frequency of the pulsations.

Ether administered to this dog produced little effect on the pressure; if anything, improved it. There was slight diminution of pressure when the animal was struggling and howling.

Ethidene was given while the animal was still to some extent under the influence of ether. The pressure gradually but very slowly fell, and, under repeated doses, reached a minimum of 20 *millimètres*. The agent being still continued, the pressure began to rise, and had reached 28 *millimètres* when it was removed. Subsequently, ethidene was again given; and, after prolonged and constant use, the pressure was gradually brought down to 7 *millimètres*, when the administration was stopped. It should be noted that all this time the respiratory movements were uninterrupted.

The same dog was used for a further set of experiments, which were prosecuted after a short interval, during which the animal partially recovered. The pressure at the beginning of this series registered 110 *millimètres*, and the variations of pressure with respiration were well marked. Chloroform being given, there was an almost immediate fall of pressure, with considerable variations, and reduction in the frequency of the heart's pulsations. The pressure remained about 70 or 80 *millimètres* for about half a minute; and then there was a very rapid fall, with great retardation of the pulsations, till the heart almost ceased. There was an interval of three seconds between two of the pulsations, of nine seconds between the next two, and of six seconds between the next; the pressure in these intervals being *nil*. The chloroform was removed when this rapid fall occurred. During this period, the respiration continued. The pulsations were now resumed; but between each pulsation the pressure was *minus*, and the pulsation only raised the mercury 10

millimètres, generally just to the basement line; while the beats occupied about one second and three-quarters. After twenty-nine of these pulsations, the breathing stopped, while the pulsations went on regularly as before. After a few seconds, artificial respiration was used, by alternately compressing the chest and leaving it to expand. This being prosecuted for thirty seconds, spontaneous respiration was resumed, and the pulsations became more marked, having a rather higher excursion than before, but still returning to a pressure of zero between each two pulsations. This continued for about forty-two seconds, when the pressure began to rise; and this went on continuously till, in a hundred and sixty seconds, a height of 100 *millimètres* was reached.

We have dwelt in some detail on this last experiment, because we regard it as one of unusual interest and importance. It is to be remembered that this animal got chloroform in the usual way, by a cloth saturated with the agent being held over his mouth and nose. He received no overdose, and the administration only lasted seventy seconds. As bearing on at least one mode of death under chloroform, the relation of the heart's action to respiration is of particular significance. The blood-pressure is enormously reduced, and the pulsations have become so unfrequent as to be virtually ineffectual, yet respiration continues. But respiration stops forty seconds after the heart has resumed, the pulsations being still, however, so ineffectual, that the pressure is even minus. We believe that the legitimate inference to be drawn is that the stoppage of respiration was not due directly to the chloroform, the inhalation of which had ceased for about forty-eight seconds. It seems likely that the failure of the heart in the first instance, and the insufficiency of its subsequent pulsations, were the cause of the failure of respiration. In such a state of the circulation, the respiratory centres would probably be insufficiently supplied with blood, and be consequently liable to cease acting. In this case, if death had occurred, it would only apparently have been due to the failure of the respiration, the primary failure being that of the heart. To what extent this may apply to human cases we do not venture to speculate.

We now resume consideration of this set of experiments. The animal was allowed to recover considerably, and the pressure had reached 106 *millimètres*, when ethidene was administered. There was a slow but steady fall of pressure, the lowest point being reached in about 120 seconds, when the height was 36 *millimètres*. Continuing the administration, there was a slight rise, up to 50 *millimètres*, when the administration was discontinued. During the administration, the cardiac pulsations were regular in frequency, with slight variations in the height of the waves, probably depending on respiration.

It will not be necessary to give our further experiments in such detail. In one set, we kept up artificial respiration by means of the pump, administering the anæsthetics by passing the air through a Wolff's bottle containing the respective agents. In these experiments, chloroform promptly reduced the pressure, which began to recover almost immediately on its removal. On continuous administration, the pressure fell much more gradually than by the ordinary method, and the lowest point reached still represented a considerable pressure, about 65 *millimètres*. The initial pressure was 132 *millimètres*. Ethidene was began at a pressure of 80 *millimètres*, the recovery from the chloroform-depression being incomplete. After prolonged use, there was a fall to 54 *millimètres*. On removal, a gradual recovery ensued, which attained to 80 *millimètres*. Ether was then given, when again a slight fall in pressure ensued.

It may here be incidentally remarked that with artificial respiration there were very exaggerated variations of pressure; these respiratory variations, however, only occurred when the animal made respiratory movements coincidently with the pumping. When, as a result of deep anæsthesia, respiration ceased on the part of the animal, the respiratory variations also ceased, although the artificial respiration was kept up.

The next experiment is a somewhat interesting one, offering in a certain sense the converse of one already narrated. The same animal was used as in the last experiment; the tracheal tube was left in, and the animal breathed through it. The anæsthetics were administered by holding a cloth soaked with the agent over the mouth of the tube. Chloroform was given, and there was an almost immediate fall of pressure, but the fall was gradual, and in 234 seconds had reached 28 *millimètres*, when the chloroform was stopped. The respiration ceased just after the chloroform had been removed. Artificial respiration was at once resorted to; but in spite of this the heart ceased beating twenty-one seconds afterwards. It is remarked, however, that although there were no indications of the heart's pulsations on the tracing, the pressure was maintained at 28 *millimètres*, and it is just possible that there may have been slight pulsations too feeble to be recorded. The pause of the heart continued for twenty-one seconds, and the pulsations were then resumed very feebly and irregularly. The pressure rose gradually to 46 *millimètres*, when a spontaneous respiration was given; then, with long intervals, spontaneous respiration was resumed, and the artificial respiration was stopped, as it was not required.

In this observation, it seems undoubted that the respiration failed first. The rapid failure of the heart is a remarkable circumstance, especially when the comparatively high pressure is

considered. It is possible that the introduction of artificial respiration may have had to do with it. The respirations before they ceased were shallow; and though the air in the lungs was saturated with chloroform, little of it would find its way into the blood in the very limited respiratory movements. The introduction of artificial respiration would at first force the saturated air rapidly through the lungs, which would be vigorously inflated, and a large amount of chloroform would be introduced into the blood.

After the animal had recovered from this experiment, ethidene was given. There was a fall of pressure; but, though administration was continued in frequently repeated doses for nine minutes, the blood-pressure only fell to 38 *millimètres*, and there was no failure of respiration. Before the ethidene was removed, the pressure had risen to 60 *millimètres*. The respiratory curve was preserved even at the point of lowest pressure, and the regularity of the heart's pulsations was not interfered with.

In this animal, the pneumogastrics were now cut, and the observations were repeated both with and without artificial respiration. It cannot be said that any essential difference was apparent in the results. The pressure fell both with chloroform and with ethidene, but the fall was perhaps not so rapid nor so great as under other circumstances. While artificial respiration was used, ethidene and chloroform were successfully pushed to a very great extent, the Wolff's bottle being heated to facilitate the evaporation of the agents. Yet the blood-pressure was not reduced to the lowest, even with this treatment, and in the case of ethidene it even began to rise under it. The effect on the respiratory variations in pressure was remarked during these experiments. In the case of ethidene, even when the agent was given to the fullest extent, the respiratory curve, though very much diminished, did not entirely disappear. In the case of chloroform, the respiratory curve disappeared completely, and that shortly after the commencement of administration. The cardiac pulsations in this case of chloroform became scarcely perceptible, the pressure, however, not falling below 40 *millimètres*. With ethidene, in the same circumstances, the cardiac pulsations remained of nearly normal amplitude.

As a result of the whole set of experiments with this dog, it may be said that the heart showed throughout a remarkable state of vigour. It only exhibited signs of giving way on one occasion, and in that case the blood-pressure was maintained at a comparatively high position. On the other hand, respiration failed during the administration of chloroform very readily. In these respects, this animal contrasts with the former one. It is quite obvious, also, that on these occasions when the breath-

ing ceased the animal would almost certainly have died, but for the use of artificial respiration.

In some further experiments which we made with another dog, using Fick's kymograph, a more remarkable result was obtained. Both respiration and heart had stopped under the use of chloroform, but by means of artificial respiration (by the pump) there was ultimate recovery, although the pulsations of the heart had ceased for a considerable time. In this case there could not be any feeble pulsations keeping up the circulation at a slow rate, as was supposed to be possible in a former case, because Fick's arrangement registers very accurately the slightest variations in the pressure produced by the heart's action. In the case of this dog also it was observed that several times, after a period of shallow respirations, the breathing stopped for a brief interval, the heart beating with considerable vigour. As respiration had ceased, no more chloroform was admitted to the blood, and after a time the respiratory movements returned. So much was this the case that difficulty was experienced in killing the dog with chloroform, and this was only effected by administering it by artificial respiration, after the spontaneous movements of respiration had ceased. By artificial respiration an additional quantity was introduced, and the heart soon succumbed.

These facts are of considerable importance, as they show the varying effects of chloroform in the same animal at different times. At one period, respiration and the heart failed nearly at the same time. At another, respiration failed, and the heart, being still vigorous, was able to carry on the circulation till the chloroform had been sufficiently eliminated to allow of the recovery of the respiration.

The facts obtained from these researches seem to us to warrant the following conclusions.

1. Both chloroform and ethidene administered to animals have a decided effect in reducing the blood-pressure, while ether has no appreciable effect of this kind.

2. Chloroform reduces the pressure much more rapidly and to a greater extent than ethidene.

3. Chloroform has sometimes an unexpected and apparently capricious effect on the heart's action, the pressure being reduced with great rapidity almost to *nil*, while the pulsations are greatly retarded or even stopped. The occurrence of these sudden and unlooked for effects on the heart's action seems to be a source of serious danger, all the more that in two instances they occurred more than a minute after chloroform had ceased to be administered and after the recovery of the blood-pressure.

4. Ethidene reduces the blood-pressure by regular gradations, and not, so far as observed, by sudden and unexpected depressions.

5. Chloroform may cause death in dogs by primarily paralyzing either the heart or the respiration. The variations in this respect seem to depend to some extent on individual peculiarities of the animals; in some, the cardiac centres are more readily affected, in others, the respiratory. But peculiarities in the condition of the same animal very probably have some effect in determining the vulnerability of these two centres respectively, and they may both fail simultaneously.

6. In most cases, respiration stops before the heart's action; but there was one instance in which respiration continued when the heart had stopped, and only failed a considerable number of seconds after the heart had resumed.

7. The use of artificial respiration was very effective in restoring animals in danger of dying from the influence of chloroform. In one instance, its prolonged use produced recovery, even when the heart had ceased beating for a considerable time.

8. Under the use of ethidene, there was on no single occasion an absolute cessation either of the heart's action or of respiration, although they were sometimes very much reduced. It can, therefore, be said that, though not free from danger on the side of the heart and respiration, this agent is in a very high degree safer than chloroform.

9. These results confirm and amplify those stated in a previous report, to the effect that ethidene does not compromise the heart as does chloroform. By the method of experimentation then employed, the effect on the blood-pressure could not be determined; and altogether the results here obtained are more exact and unequivocal.

It may be added that since the last report ethidene has been given to a number of patients of all ages, with results which may be described as satisfactory. Given freely at first, it produced anæsthesia as rapidly as chloroform, and the effect could readily be kept up by comparatively small subsequent doses. The only drawback is that in some cases it produced vomiting; but it has not been determined that it does so more frequently than chloroform, over which it has the further advantage of producing less excitement and being more agreeable to the patients.

Isobutyl chloride was given to three patients, but it produced considerable excitement and proved an imperfect anæsthetic.

In concluding this report, the Committee would express the belief that, so far as purely physiological tests go, ethidene has proved itself as efficient an anæsthetic as chloroform, and a much safer one. It is not asserted that this agent ought at once to replace chloroform in practice; but it is believed that a very strong case has been made out for an extensive trial of it. It is impossible for the Committee, having numerous other

engagements, to give the agent that extended trial in actual practice which is desirable; and they would ask those who are engaged in practice to use it in their cases, and to report the results they may have obtained.

It appears that ethidene was first employed as an anæsthetic by Dr. Snow. He administered it in fifteen cases, with good results. (See Snow on Chloroform, etc., last paper, published in 1858.) In 1870, it was used by Liebreich and Langenbeck in Berlin. (Berlin Klin. Wochenschrift, 1870, p. 401.) In 1871, two papers appeared; one by Sauer, in the Pharm. Centralblatt, vol. xiv., p. 140; and the other by Steffen, in Deutsche Klinik, No. 44, p. 398. Sauer mentions one case of death in a patient suffering from heart-disease. In thirty-three cases, two vomited, and two suffered from nausea and headache. In 1872, Steffen publishes another paper in the same journal, p. 358, in which he gives details regarding twenty cases; and he states that the results were satisfactory. (See also Jahresb. der Medicin., 1870, 1871, and 1872, where abstracts are given.) Snow states that the difficulty in obtaining ethidene pure may prevent its general use. That difficulty is now removed; and there is little doubt that, if required, ethidene may be made in a state of purity and at a moderate cost.—*British Medical Journal*, June 21, 1879, p. 921.

78.—ON CHLORAMYL AS AN ANÆSTHETIC.

Chloramyl, a combination of pure chloroform and nitrite of amyl, has recently been tried as an anæsthetic, at the London Hospital, by Mr. Rivington, Surgeon to the Hospital. The first patient to whom it was administered was a healthy man, and the operation merely the slitting up of a sinus. The patient inhaled the drug freely and comfortably, with no symptom of choking; the pulse increased almost immediately in volume and rapidity; the respirations were more frequent and less deep. In three minutes, the patient began to struggle, and, within four minutes of the commencement of the administration, the pulse suddenly failed, so as for a moment to be hardly perceptible; the respirations became hurried and shallow; the jaw appeared to be closed by spasm; the lips were blue; the eyes staring and suffused, the left pupil much dilated, but the right of moderate size, (about the dimension of a No. 8 catheter); the breathing was very noisy and stridulous, as if due to laryngeal spasm. With difficulty the mouth was forced open. These symptoms passed off rapidly, and in about the space of two minutes the patient came to himself, without passing through the talkative stage usually observed when chloroform is given. The slight operation needed was performed while he was quite conscious. He himself thought that the anæsthetic had caused

him to feel the pain less acutely. The next patient anæsthetised was a young woman aged 25, suffering from extensive warty growths of the vulva. Mr. Rivington cut away the growth, arresting hemorrhage by pressure and the occasional application of the actual cautery. She was in good health. She inhaled the chloramyl comfortably, and in five minutes was fairly under its influence. The pulse remained throughout full and regular, the respiration easy. As in the previous case, she regained consciousness without passing through the stage of disquiet usually observed. The third operation was for the removal of necrosed bone from the hand; the patient was a healthy man. In six minutes he was perfectly anæsthetised. His pulse during the first minute became intermittent, the intervals of intermission decreasing in frequency until the third minute, when the pulse was perfectly regular. The respirations were throughout easy. The patient struggled a great deal, but came to himself without any display of restlessness or talkativeness. In each instance, the patient was free from any cardiac mischief. The drug was administered in the same manner as is adopted at the hospital for the administration of chloroform, but the quantity used was greater. It was observed that, when once the patient was well under the influence of the chloramyl, small quantities of the drug were sufficient to keep up the narcotic effect. All the patients recovered comfortably without vomiting or other bad result. In the two latter patients, the pupils remained throughout quite equal, the eyes turned up, with lateral mystagmus, the globes retaining perfect parallelism. The drug was obtained from Bass, Brothers and Co. Chloramyl was first advocated by Dr. R. Sandford, in a letter to an American journal. From experiments upon animals, he has come to the conclusion that this combination is far safer for general anæsthetic purposes than chloroform uncombined, and, "so far as tried, it seems to be fully as safe as sulphuric ether, and far more pleasant in its administration, possessing all the advantages of pure chloroform without its dangers." He states that, "in administering chloramyl, the patient's face becomes flushed much sooner than with chloroform, but press the drug right along, and the countenance does not become pale. Both heart's action and respiration are kept up thoroughly throughout the anæsthesia." Dr. Sandford alleges that chloramyl prevents the approach of danger both by syncope and by asphyxia. The formula he uses is: Squibb's chloroform, lb.j; nitrite of amyl, two drachms. He suggests that the amount should be diminished in long and tedious operations. Mr. J. T. Clover, in reviewing Dr. Sandford's communication in the January number of the London Medical Record, stated that he made a trial of this mixture in ten cases. The anæsthesia was quickly produced, without

much excitement in any case; but three suffered nausea afterwards, and two of them vomited and remained for an hour much in the same condition as if chloroform alone had been given. It appears to be similar in its action to that of a mixture of chloroform and ether; but as the vapour is less pungent, the patients generally breathe it without resistance. It was much too soon, Mr. Clover thought, to pronounce upon its relative safety.—*British Medical Journal*, April 26, 1879, p. 640.

79.—ON CROTON CHLORAL AND NITRITE OF AMYL.

By Dr. R. RIDDELL, Belfast.

My first experience of croton chloral, as it is wrongly called, was in the person of a near relative, who for years had suffered from severe attacks of headache—so severe that at the times of attack she was quite prostrated. The constant description she gave to the medical men she consulted was that of feeling as if an iron band were round her head and crushing it. These attacks were recognised by all as neurosal in origin, and for years it was attacked by all the “great guns” of the Pharmacopœia until its resources were exhausted—the bromides were tried up to so-called “heroic” doses; chloride of ammonium, iron, arsenic, hydrobromic acid, sedatives and stimulants; quinine, in small or large doses, only made it worse—all of no effect for good. I was reading some remarks in the Practitioner concerning croton chloral, and it seemed to me that it might do good in this case. I suggested it to her, and she consented to take it, but with no idea or hope of any relief, for she had suffered from these attacks for more than twelve years. I began with 5-grain doses twice daily and 10 grains going to bed, dissolved in spirits of wine and glycerine, with a little acid and syrup of orange to cover the flavour. The good effect of the drug was seen at once; the attacks came at longer intervals, and were less severe, then ceased altogether; and now for upwards of seven months she has had only two attacks—one at a time of great mental distress, and one when she stopped taking the drug, about two months since, when she resumed taking it, and has not had an attack since. She now takes 5 grains per diem (generally at night), is hearty and well, and has a good appetite. Her age, I may say, is sixty-three. The 5-grain doses only caused a slight drowsiness, but the 10 grains at night, by relieving all pain, gave her a good night's rest—in fact, she now enjoys better health than she has done for years.

Since that time I have used it largely—sometimes failing, sometimes relieving—till, by keeping an account of all my cases, it began to dawn on me which were benefited by the drug. Since then the number of cases relieved (some permanently) has

increased. These cases are—headache in females arising from mental distress; those cases of headache so frequent at the menopause—in fact, all those called neuralgic, except a few arising from internal mischief, are benefited, and, in many instances, cured. In that distressing species of neuralgia called *tic-douloureux* I have found it in many cases acting like a charm. Of course I do not include any arising from cranial or intercranial causes. I have tried it in neuralgia of the ovaries, but no good resulted. In insomnia it is not so reliable as the hydrate, but in some cases where the loss of, or inability to, sleep is accompanied by a weak or fatty heart, it is to be preferred, as it has no weakening effect on the central organ of the circulation. In one case of delirium tremens, where the circulation was very feeble, the combination of croton chloral with digitalis had a wonderful effect, and it seemed as if the drugs could be given together in much smaller doses, to produce the same results, than singly. In this I pushed it from 10 to 30 grains every three hours, with drachm and 2-drachm doses of the infusion of digitalis. In pain arising from caries of teeth I have found it useless in most cases, and in all inferior to Richardson's "*tr. gelsemini*;" but in one case of a nervous young lady, by giving her two 10-grain doses I was able to extract a tooth next to painlessly, to her great satisfaction. You will notice in all these cases it is in affections of those parts supplied by the fifth pair of nerves that it is of most use; but, to be of service, you must give the drug in far larger doses than prescribed in the *Pharmacopœia*—for adults, 5 grains three or four times daily, gradually increasing if required; if stimulants are wanted, dissolve it in rectified spirit; if not, dissolve it in glycerine. In all cases complicated with haemorrhoids, give glycerine. If anæmia exists, combine it with iron, or, which I believe better, arsenic; then gradually lessen the chloral. In all cases I have found it better to give it in solution than in powder or pill.

Since last meeting, when it was intended that I should have read these notes, some cases of great interest have come under my notice, in conjunction with my observation on the fact that croton chloral exerts most influence on painful affections of the superficial nerves of the face and its adjuncts. On the 18th December I was consulted by J.A., aged twenty, a striker in a smithy, for "something in his eye." I made careful examination, and could find nothing, although there was intense photophobia—tears streaming through the "clenched" eyelids. I dropped in atropia and counter-irritated his temples, which I have often seen doing good, but to no purpose. Another examination discovered a minute abrasion of the conjunctiva. I dropped in oil and atropia again, rubbing the extract of belladonna over the eyebrows; no effect. It struck me to try the chloral, seeing

its power over these nerves, and it was most wonderful. I gave him 10 grains at once, and repeated that dose in an hour, when considerable relief was experienced. I repeated the 10 grains in two hours more and the pain was entirely gone, though for precaution's sake I gave him 5 grains every three hours for a day or two.

J. F., brush-maker, consulted me on the 21st for facial carbuncle. You all know the terrible pain suffered in that disease. To control that pain and give sleep I gave opium, Indian hemp, and the hydrate of chloral. Opium gave relief at first, then failed on account of stomach derangement. The other two were useless, or nearly so. Following out the same line of thought as in the former case, I gave the croton in 10-grain doses. The effect was simply marvellous. Giving 10 grains every three hours, the disease went through its different stages, and very seldom would the patient have known of anything the matter from the sense of feeling.

The last case was a lady who has been under my care a long time, and who for months suffered from neuralgia, coming on at 12 o'clock, noon. In fact, she said (a little bow-stretching, you will say) "I can set my watch by the attack." Considering the periodicity, I gave arsenic, nux vomica, iron, and a whole array of tonics. I then (since last meeting) gave the chloral; and after two days she has had only one attack, and that a mild one.

I do not vaunt croton chloral as a specific—I do not believe in the term except in one instance—I do not compare it with, or fight its battle against, any other drug—no physician should have a "favourite" drug or prescription—but in my hands it has been useful in relieving pain, and I use it as I would any other instrument for good.

I have only one or two facts concerning nitrite of amyl to bring before you. The first case is that of a young married lady with menorrhagia, who, at the times of the catamenia, suffered from terrible headaches—always relieved, however, by hot spirits and water. She herself and her friends were desirous of some other remedial means than spirits. I came to the conclusion that the pain was anæmic in origin; and the nitrite of amyl possessing the dual power of increasing the heart's action and dilating the intercranial vessels, I thought it would do good; so I mixed the amyl with equal parts of eau-de-Cologne, and ordered her to use 5 or 6 drops of the mixture dropped on a handkerchief and inhaled by deep inhalations. It succeeded perfectly, and ever since she has never been without that combination—all other remedies used to put an end to that infliction having failed.

One night I was called out to see a case—a woman aged about fifty-four—"heart disease" the friends called it. When

I arrived I saw at once it was a case of asthma. She was in a terrible paroxysm; leaning forward on her elbows, with flushed face and dilating nostrils, she was gasping for breath. I thought of the amyl, and fortunately had some with me. I poured out 5 drops on my hand, rubbing them over the palm, and held my hand to her mouth; when she had inhaled 10 drops she could breathe naturally. A course of arsenic effectually cured her.

J. L., a clog-maker, consulted me the day before Christmas, 1877. He suffered from asthma, and the attacks were so frequent that he was off work more than half time, and the paroxysms were severe. In every case the amyl checked it at once; and now, nearly twelve months having passed, he informed me yesterday that he had only lost one week altogether, and that was through exposure to cold. I may say arsenic was the drug I gave him during the intervals.

I have had abundant opportunities of trying it in sea-sickness, but only in a few instances it did good. But in that vile sickness one is thankful for small mercies. In one of my voyages one of the passengers had brought some chloroform on board for toothache. I was summoned in haste to her cabin, and found her quite insensible, pulse not perceptible, and evidently dying. Fortunately, though the nitrite was not in the ship's surgery, I had brought some to experiment with. After inhaling a few drops the heart's action was quickened and strengthened. Three or four times we had to repeat the inhalations before she was out of danger. It is so antagonistic to chloroform that I am of the opinion that it should be at hand at every operation in which chloroform is given. I throw out the hint for any of the surgical staff of the hospitals who may be present. In opium-poisoning I have derived no benefit from it—I suppose for the reason that that poison kills by stopping the respiration first. In fainting attacks from weakness of the heart, or brought on by witnessing an operation, or a cut, it is of great service. It has often an equally good effect in spasmodic and cardiac asthma, though its action must be different in the two diseases. In the one it must relieve by lessening the spasm of the air-vessels, in the other by increasing the power of the heart. How it does this I do not know; I am only glad to know that it is a fact.

In conclusion, I have brought before your notice what I believe to be two very valuable medicines, exercising relieving and curative effects on two distressing maladies—neuralgia and asthma, also acting as an antagonist to chloroform.—*Dublin Journal of Medical Science*, April, 1879, p. 346.

80.—ON NITRITE OF AMYL IN CHLORAL POISONING.

By Dr. J. G. SINCLAIR COGHILL, F.R.C.P.Ed., Physician to the Royal National Hospital for Consumption, &c.

The following case indicates so markedly the value of nitrite of amyl as an antidote in poisoning by chloral-hydrate, and otherwise presents so many other points of interest in connection with the symptoms produced by overdoses of that now much used and much abused drug, that I have yielded to the request of some of my medical brethren to make it public.

A. B., aged 62, of spare habit, was a frequent and intense sufferer from gout, in seeking relief for which he had unfortunately become somewhat addicted to alcoholic stimulants and narcotics generally. Late on the evening of the 23rd of April, after a liberal potation of whiskey, he took a large, but unfortunately an unascertained dose of his favourite anodyne, chloral. The dose must have been a very large one, for within a very few minutes he became completely insensible. Fortunately, medical assistance being at hand, the case received immediate attention from the very commencement of the symptoms, which, however, became so alarming, that in about two hours I was sent for. I found that artificial respiration had been kept up for some time, but only with the effect of inducing feeble, superficial, gasping respirations, at the rate of four per minute. The surface was cold and deeply cyanosed, and the pupils strongly contracted to the size of a pin's head. The pulse, however, was 80, full, but soft and compressible. I had the tongue at once pulled forward, and maintained in that position with forceps. Taking the state of the pupils as an indication, and remembering Liebreich's theory of the decomposition in the system of chloral into chloroform, I immediately administered by inhalation from a handkerchief about twenty drops of nitrite of amyl. The effect was immediate. Within two minutes warmth had returned, even to the extremities, and the surface had assumed the hue of health. Within ten minutes, the respirations had become much deeper, reaching nine per minute, and afterwards gradually increased up to twelve. The amyl had to be repeated in a smaller dose in about two hours, with permanent effect. At 9.30 next morning, the general condition was found to have improved somewhat, but there was no return of consciousness; and an attempt to give fluid nourishment by mouth had produced great embarrassment of breathing. I ordered an enema of brandy and Liebig's extract in arrowroot to be given, and repeated every two hours. After the second enema, the patient became quite sensible, recognised and spoke to those around him, and swallowed some food with little trouble. I saw him again at 6.30 p.m., when the water was

drawn off normal in amount and quality. I am informed that he continued to improve until 9 p.m., when he suddenly started up as if from sleep, with staring eyes, threw up his hand, uttered a cry, and fell back dead. I am inclined to think this fatal result might possibly have been averted by a more copious and frequent stimulation *per anum*.

The principal points of interest to be noted in this case are the extreme contraction of the pupils; the intense affection of the respiratory, the complete immunity of the circulatory, system; the rapid recovery of warmth and colour, with restoration of the respiratory function under the influence of nitric of amyl; the return of consciousness in response to stimulation *per anum*; and the sudden failure of the heart's action, which proved immediately fatal.

In cases of poisoning by chloral-hydrate, very opposite observations are on record with reference to the state of the pupils, and also as to the relative extent to which the action of the heart and lungs is influenced by the drug. Mr. W. Sedgwick, who has made a special study of the subject, states that in most instances the pupils are contracted; while Dr. Cleveland, and especially Dr. B. W. Richardson, report the contrary to be the invariable condition. I believe that the explanation of these apparently discrepant phenomena must be sought for in the difference in the amount of the drug swallowed, and the corresponding rapidity of its action. When chloroform is administered in excess *too rapidly*, it seems to prove fatal by paralysing the respiratory centres, while the pulse remains comparatively unaffected, the pupils being *contracted*; but when chloroform inhalation is kept up *too long*, so that the drug accumulates slowly in the system, the heart first yields to its influence, and succumbs earlier than the respiration, and under these circumstances the pupils will be found *dilated*. I have ascertained these conditions, both experimentally in the lower animals, and from a large experience of chloroform administration commenced twenty-two years ago, while assistant to the late Sir James Y. Simpson. A parallel discrepancy in symptoms may be noted also in cases of delirium tremens, where the phenomena of the attack have been developed as a result of prolonged drinking to excess, or from one deep debauch.

Liebreich, the discoverer of chloral-hydrate, believes that it acts on the system by being resolved into chloroform from decomposition in the presence of an alkali; and although this opinion is purely theoretical, yet it must be admitted that there are marked and close resemblances both in their physiological and therapeutic effects. This would at once explain why nitrite of amyl should be the best antidote in chloral-poisoning, much more certainly than strychnia, which has been proposed as its

antagonist; while, strangely enough, nitrite of amyl itself is proposed by Dr. B. W. Richardson as the antidote to strychnia poisoning. May it not be that nitrite of amyl will prove the appropriate antidote when the drug has been administered in such quantity as to act rapidly on the respiratory centres, *with contracted pupils*, and that strychnia should be given when the drug has acted slowly as a cumulative poison when the heart has succumbed, *and the pupils are found dilated?* — *British Medical Journal*, June 28, 1879, p. 969.

81.—CASES OF SUDDEN DISCONTINUANCE OF OPIUM AFTER DAILY USE IN LARGE DOSES FOR YEARS.

By Dr. ROBERT H. LLOYD, Lambeth Infirmary.

The following cases will illustrate that it is possible, in many cases, to discontinue suddenly the use of opium without any evil results.

J. H., aged forty-five, who had had sunstroke whilst in India in 1856, and who has occasionally had epileptiform fits, was in the habit for three or four years of taking an ounce of laudanum every morning, without which he was unable to do any work. He took it at first in twenty-minim doses, to relieve rheumatic pains, but found it such a comfort that he soon increased the dose to one ounce.

On admission he presented all the symptoms of chronic alcoholic poisoning—viz., insomnia, morning sickness, frightful dreams, &c. The patient was placed on a nourishing diet, two grains of quinine were given three times a day, and a strict watch was kept to see that he took no laudanum. At the end of a fortnight he was in good health, his appetite was good, and, beyond his being unable to sleep for the first few nights, the discontinuance of the drug caused no ill effects whatever. This took place in January 1875, and he has been frequently seen since. He has not touched laudanum since, and has had no recurrence of the epileptiform fits.

Case 2.—S. J., aged seventy-five, widow of a soldier, who was admitted in 1874, with a very extensive ulcer of the leg, contracted the habit of eating the extract of opium thirty years ago, when in India. In this case the use of the drug was discontinued suddenly, and no bad symptoms followed. The patient's ulcer healed up more rapidly than before.

Case 3.—W. G., aged forty, a musician, was under observation with very painful disease of the bladder and a stricture, for which he had been having for months three grains of morphia injected hypodermically every day. The stricture was treated by Holt's method, and the morphia was discontinued at once, the only evil result being a few sleepless nights.

Case 4.—G. E., aged twenty-five, had been in the habit of taking from half a teaspoonful to one teaspoonful of laudanum two or three times a day for the past four or five years, whenever he felt low-spirited or “out of sorts.” He had taken that quantity regularly for the past six months. The use of the drug was discontinued at once. Some time after he declared the result to be increased weight and a general feeling of thorough sound health. In this case no quinine was given.

Patients who have been in the habit of taking opium in large doses for years are frequently seen at the Lambeth Infirmary, but Dr. Lloyd affirms that he has never seen any evil results follow the practice of discontinuing the drug suddenly. “I always,” he says, “place them on a nourishing diet, without stimulants, and administer quinine in one or two grain doses.”—*Lancet*, June 7, 1879, p. 807.

82.—A CASE OF OPIUM-POISONING TREATED BY THE
SUBCUTANEOUS INJECTION OF ATROPINE.

By Dr. J. E. EDDISON, Physician to the Leeds Infirmary.

M. D., aged thirty-five, who was admitted at 9 p.m. on Feb. 13th, was said to have taken about six drachms of tincture of opium an hour before. On admission he was able to answer questions, but manner irritable; pupils contracted. He refused to allow the stomach-pump to be applied. Twenty grains of sulphate of zinc were administered at once.—9.40 p.m.: No sign of vomiting; patient drowsy, and unable to stand without assistance. Stomach-pump used, and about twelve ounces of brownish fluid withdrawn. Strong coffee was injected into the stomach, and the patient was “walked” about the corridors between two assistants. 11.20 p.m.: Patient worse; more sleepy; could only be roused by violent shaking and the application of a wet towel. Pulse 120; respiration 15; pupils almost pin-point. One-tenth of a grain of sulphate of atropine injected, and patient allowed to lie down. Immediately the condition slightly improved, and continued up to 12.20 a.m. on the 14th, when he became utterly unconscious, and incapable of being roused even by the most violent means (wet towels, faradism, &c.). Pupils still contracted; pulse feeble and rapid; respiration 12, and falling. A quarter of a grain of sulphate of atropine was injected subcutaneously at 12.20.—12.40 p.m.; Patient somewhat better. Pulse about 130; respiration 18; pupils dilating. No return of consciousness; extremities cold; sleep more natural.—1.10 p.m.: Respiration suddenly sank to 12, but rose again to 20 after artificial respiration had been kept up for about ten minutes. Pulse good; surface of the body warmer. From this time up to 8 a.m. the patient slept,

and though attempts were made at intervals to rouse him, they were met with only partial success. At 8 o'clock he awoke, was able to answer questions put to him, took some breakfast, and from that time had no further trouble, and continued quite well up to the date of his discharge from the hospital (Feb. 25th).

Remarks.—In this case, as in so many others of the kind, it is impossible to say how much opium was actually absorbed; and it is equally impossible to say positively that the man would have died if the atropine had not been used. The case may, however, be taken for what it is worth, as a contribution to the disputed question whether we ought to administer atropine in cases of opium-poisoning. The time when the opium was taken is known; the symptoms gradually increased in severity; they were unaffected by one-tenth of a grain of sulphate of atropine; and all the usual means were adopted without any apparent prospect of saving the patient's life. Then, about four hours after the opium had been taken, and when the symptoms of poisoning were worse than they had been, and when the patient was absolutely unconscious, and could not be roused, one-fourth of a grain of sulphate of atropine was injected. Within twenty minutes the respiration went up from 12 to 18, and the pupils began to dilate, and, except that respiration failed for a short time, the effect of the opium gradually passed away, and in seven hours more the patient awoke, and took some food. With the exception of a slight bronchial catarrh, he remained perfectly well until his discharge. It may be said that he would have recovered without the atropine; it remains, however, that the change for the better took place almost immediately after the atropine was injected, and just when the evidence of its action on the pupils became manifest. Without making too much of a single case, it may be fairly said of this one that it in no way supports Dr. John Harley's opinion as to the effect of doses larger than 1-96th of a grain; nor, indeed, does it support his views as to the use of belladonna in opium-poisoning at all, for it seems evident that, whether the tenth of a grain did any good or not, the quarter of a grain injected an hour after certainly did no harm. It should be remembered that faradism, local stimulation, heat, forcible dragging about, as well as frequently renewed artificial respiration, were all fully tried, apparently without any permanent good results, and there really appears to be every reason for concluding that the atropine had a great deal to do with the patient's recovery. In speaking of the use of atropine sulphate in opium-poisoning, Dr. John Harley says distinctly: "If larger doses [than one ninety-sixth of a grain] be given, or if smaller doses be too often repeated, the beneficial effects of belladonna will be converted into a depressant and narcotising influence." The

solution used in this instance was prepared at the time by the house-surgeon, Mr. Brown, who conducted the treatment, and there is no reason for supposing that it was not of usual strength. Yet in face of so clearly-stated an opinion given by Dr. Harley as the result of many observations, it is not surprising that many men still use only the ordinary means in cases of opium-poisoning, and prefer to avoid the risk of being accused of increasing the poisonous action of the opium. In most cases of this kind we can only guess at the amount of opium that has been absorbed, and rules stating that such and such an amount of atropine will neutralise such and such an amount of opium are, unfortunately, not of much practical value. We require to know how much atropine should be given in the presence of symptoms which can at best only be described in a more or less definite way. The dose in this case seems to have been exactly appropriate, and it certainly cannot be said that the atropine did any harm or in any way increased the depression of the narcotism.—*Lancet*, June 14, 1879, p. 843.

83.—ON INJECTION OF AMMONIA INTO THE VEINS AS A
MEANS OF RESUSCITATION IN ALCOHOLIC AND
NARCOTIC POISONING.

By ROBERT HAMILTON, Esq., F.R.C.S., Senior Surgeon to the
Royal Southern Hospital, Liverpool.

The idea that the ammonia generated in the blood in the process of nutrition is for the purpose of acting as a solvent—that is, for the holding together the other constituents—seems probable from the fact that the rapid escape of ammonia by evaporation from freshly drawn blood, and its dissipation in the atmosphere, proceeds *pari passu* with the coagulation of the blood. It seems to show that they stand in the relation of cause and effect.

The immediate tendency of the blood to coagulate as soon as it is withdrawn from the body—that is, for its component parts to separate—certainly leads to the conclusion that its several constituents are only very lightly held together, even when within the body. That temperature, motion, and the exclusion of air, either separately or in conjunction, are the chief agents in sustaining the fluidity of the blood is very doubtful; for when we see the wide range of temperature, with the sometimes sudden alternations to which the blood is subjected, and when, again, we find it can also more than double its speed without influencing its consistence, we can hardly think that these two—motion and temperature—are the controlling powers sustaining its fluidity.

Then, as to the exclusion of air, that cannot of itself be the

cause, as the following experiment shows. The jugular vein of a dog was tied, and a ligature passed round it in two places. The blood enclosed between the two ligatures was found in less than an hour to have undergone decomposing changes, for, on a slit being made in the vein, an escape of air and partly coagulated blood followed.

To what extent the impairment of this essential condition for carrying on of life—namely, the perfect fluidity of the blood,—occurs, and under what circumstances, have not been made the subject of investigation; but that there are some forms of dying whose *modus operandi* is through a gradual thickening of the blood, producing a slow and yet slower circulation until complete stoppage occurs, is probable, although an examination of the bloodvessels after death does not reveal it. It is no sufficient answer to this supposition to say we ought to find them choked and full of clotted blood. Arguing from the fact that two minutes suffice for the complete coagulation of freshly drawn blood from a living subject, and that the subsequent steps in the process of disintegration of that blood are equally rapid, we ought rather to conclude that the condition of the blood in the veins and capillaries of a person who has been dead but an hour is probably very considerably altered from what it was at the moment of death, and that the arteries being empty, and viscid blood being found in the veins, are no evidences of what was the actual condition of the circulating fluid when the heart ceased to pulsate and the lungs to respire.

In the beginning of 1878 I was making some experiments with fresh animal blood, and repeated many of those of Dr. Richardson's. The power of ammonia to suspend coagulation was fully confirmed in these experiments. It suggested the thought that the elimination of latent ammonia, by its ready volatility, was the first step in the process of disintegration of the blood. But it was not until the following case occurred that the further thought arose that possibly the action of some poisons when taken into the system was that of stiffening or thickening the blood, if such a term might be applied to a diminished fluidity, the result of an arrested vital force leading to an arrested supply of an ingredient necessary to the maintenance of fluidity. Might not that ingredient be ammonia?

In apoplexy and in fractures of the skull with effusion of blood, where it was formerly thought that the clot of blood was by its pressure alone the cause of death, it is more probable that a progressive clotting of the blood in all the vessels in the neighbourhood sets in, and the circulation of the blood becomes arrested from this cause over an ever-widening area until death ensues. The engorged state of all the bloodvessels of the brain found after death favours this view. In such cases death has

occurred too soon for the further disintegration of the blood and the escape of its serum, but there are numerous other diseases where the arrest of the circulation or an impediment to it leads to the separation of the fibrin and serum and the infiltration of the latter into the tissues or into cavities. Serous apoplexy, ascites, and anasarca, are familiar instances.

The property of holding together in uniform admixture the constituents of the blood must be lodged in certain elements which are themselves the product of nutrition. This being so, we may go further, and assert that there occur certain conditions of the blood as the result of abnormal nutrition or of blood-poisoning where these necessary elements are deficient, or at least not present in sufficient quantity to hold intermixed the other constituents. Is this what happens in alcoholic and narcotic poisoning? Are nitrogen and hydrogen, combined in the form of ammonia, the elements wanting? If they are, it affords an explanation of the good effect of the introduction of the latter directly into the blood in the case now to be related.

S. C., aged fifty-two, living in a neighbouring street. I was called to her on March 6th, 1878, at 7.30 a.m. I found her lying on a sofa insensible, having all the appearance of stupor from drink. Her history was that of a confirmed drunkard. Her face had a very bloated appearance. She had risen an hour or two before, and gone to an adjoining public-house, and been supplied with drink—rum or brandy,—had come home and laid down on the sofa, and the children after a time being unable to rouse her, sent for me. There were contusions on various parts of the head and face, which gave rise to a suspicion of ill-usage as well. By shaking and shouting to her she would partly open her eyes. Mustard plasters were applied to neck and calves, and attempts made to get her to swallow a little coffee, but without success.

I then had her removed to the hospital, to which she was conveyed perfectly insensible in a cab. Here galvanism and the stomach pump were freely used, without rousing her in the slightest. She was then removed to bed in a private ward.

At 11 o'clock I saw her again. She was quite comatose, pupils contracted, pulse feeble and flickering, and skin moist and perspiring. All attempts at rousing her failed.

At 1 p.m. I saw her again. The pulse had now become so feeble as to be scarcely perceptible; her lips and tongue were livid and cold; there was a frothy secretion of mucus from the mouth, and peculiar changes of colour passed over her countenance, which seemed to indicate the nearness of the end. I left her with the impression that in a few minutes pulse and breath would stop. Going down stairs with Mr. Gwatkin, the senior nouse-surgeon, discussing the case, he threw out the

suggestion to try the injection of a little ammonia into one of the veins, a proposal to which I gladly acceded, and having obtained some strong solution of ammonia from the dispensary, I returned to the ward and injected with a hypodermic syringe ten drops into the medio-cephalic vein of the right arm. The effect was striking. In a few moments she moved slightly—an uneasy, restless motion, and soon after, on being shaken and spoken to, partly opened her eyes and turned her head. The most marked change was in the pulse and mouth. The former, which was not to be felt just before the operation, could now be detected, and after a time counted, whilst the mucous membrane of the mouth and tongue became almost immediately of a natural colour.

She slowly recovered consciousness. In the evening, when I saw her again, she had so far rallied as to be able to swallow. She complained much of her arm. The skin over the injected vein subsequently sloughed. Her recovery was slow, due to the intemperate life she had led, and to the ill-usage of a brutal husband, every part of her body being covered with bruises and contusions.

I have delayed the publication of the above remarks for several months with the hope of being able to give further illustration of the effects of ammonia injection. Quite recently a woman was brought into the hospital poisoned by drinking carbolic acid. The case was apparently hopeless, when the house-surgeon injected solution of ammonia into the veins of her arm, and the woman recovered.—*Lancet*, August 2, 1879, p. 157.

84.—ON ARSENICAL WALL-PAPER POISONING.

By JABEZ HOGG, Esq., M.R.C.S.Eng., Consulting Surgeon to the Royal Westminster Ophthalmic Hospital.

On the 7th of April, I read a paper at the Medical Society of London, on Inflammation of the Eye and Injuries to Health by Arsenical Wall-paper Poisoning. Since that time, I have met with several typical cases of wall-paper poisoning of considerable interest, the publication of which will, I believe, have the effect of stimulating further inquiry into an important question of hygiene.

Case 1.—Fainting, Sickness, Cramps, and other alarming symptoms of Acute Poisoning.—The following interesting and remarkable case forcibly illustrates the rapidity with which the poison may be absorbed by one who is susceptible to its influence. In my opinion, the acute nature of the symptoms experienced by the patient were mainly due to a saturated atmosphere of arsenuretted hydrogen, arsenical dust being of a less diffusible

nature. The humid state of the surrounding air, which was at the same time the subject of remark, possibly facilitated the evolution of the poisonous gas; and the several causes, combined together with the overheated state of the room, from a desire to make the visitor as comfortable as possible, brought about the alarming form of illness I am about to narrate. I may further remark that, as I am anxious not to weaken the tersely graphic account of the seizure with which Mr. E. H. Corbould has furnished me, I need, I am sure, offer no apology to this gentleman for relating the history and symptoms of his case in his own words.

"I was summoned," he says, "a few years ago, to Osborne, to execute a particular work in a very short space of time; and it therefore became necessary to lodge and sleep as near as possible to the palace, in order that I might resume my work early, and leave it late. To enable me to do this, I took a room at the nearest hotel. In a day or two, I began to feel ill, and found I was suffering from all the symptoms of a severe cold. This I attributed to the damp state of the walls of the bedroom in which I slept, the paper of which was much discoloured by the action of the damp. Her Majesty, on hearing of this, in her most graciously considerate way, gave instructions that I should at once be lodged at the Victoria Cottage, just outside Osborne, and that a fire should be lighted in the bedroom some hours before I took possession of it; in short, that I was to be made as comfortable as the palace could make me. The result was that at night I found the room exceedingly cosy. The bed was everything a gentleman could desire. It was well covered with Witney blankets of the newest and the best kind; and I was informed by the servant that, in the event of my requiring more, I should find a couple of thicker blankets folded at the foot of the bed. When, however, I felt the quality of those on the bed, and which were like so many good sheepskins, I said it was not very likely that I should require the extra ones. The room, too, felt deliciously warm in every part. I soon got into bed, but had not been there many minutes when a coldness seized me. I was soon obliged to resort to the blanket reserve at the bottom of the bed, and, dragging forth one, I threw it over me, and once more tried to settle myself down to sleep. I still, however, felt very chilly, and in a few minutes was glad to seize the other blanket, and fold it double over my legs. Withal, I could not get warm, and my teeth chattered; and I resolved to try the effect of a large and well-lined Spanish cloak which I had thrown over a chair near the bed. I made an effort to raise myself in bed, but to my horror, found I had not strength enough left to move my legs and get out. The effort produced a violent pain of my bowels. I felt for a moment as if some

one were twisting them, just as you might take a jack-towel between your hands, and wring it out. The severe pain was followed by a dreadful sickness; and, during the paroxysm, I must have fainted, and from sheer exhaustion fallen asleep, for I remember no more of what passed. At eight o'clock in the morning, the servant knocked at the door and awoke me out of a deep sleep, and in a feeble voice I called to her to bring me a cup of tea. In about ten minutes, she returned with it, and then for the first time I opened my eyes; and, seeing a very brilliant green wall-paper before me, I excitedly exclaimed, 'Why, it's arsenic!' The girl was surprised, and, innocently enough, assured me it was no such thing, but some of the best tea. I explained to her that I alluded to the colour of the wall-paper, and she hastily departed. I omitted to state that I took a small glass of hot brandy-and-water before going to bed; and I firmly believe, but for this, I should not have survived that dreadful night.

"On first getting out of bed, I could scarcely stand; indeed, I staggered about like a drunken man, and it required a considerable effort on my part to dress. This I at length accomplished, and opening the door of the room, I stepped out into the passage where there was an open window. The fresh cool air of the morning seemed to give me new life, and in few seconds all unpleasant sensations vanished as if by magic, and the moment I got into the air I felt perfectly well.

"Upon my arrival at the palace, I found the Queen waiting for me. She remarked that I was late; and I was therefore obliged to excuse myself, by confessing that I had been very ill during the night, and at the same time I intimated that my illness had been caused by the poisonous green wall-paper of my bedroom. Her Majesty was greatly startled by my statement; and after an expression of deep concern and sympathy with me, at once commanded an attendant to have a piece of the paper stripped off the wall and brought to her for the purpose of being examined and tested. This was done, and the paper proved to be as I suspected—highly arsenical. With a promptness and considerateness for the comfort of those about her person so proverbial of Her Majesty, the paper was immediately stripped off, not only from this identical room, but from all other rooms in and about the palace; so that, by her prudent and prompt command, a stop was at once put to any future risk of health or life to anyone who might rest under the roof of the royal palace; and also that it might act as an example to her subjects to suppress decorations calculated to insure a torturing death. Certainly I was not a little touched by the deep concern evinced by Her Majesty to have that which was most pleasing to look upon utterly destroyed. I must confess, however, that that

terrible green is not likely soon to be forgotten by anyone who has suffered from its effects, and I therefore think the public should be put on its guard against its use, otherwise I fear many a valuable life will continue to be sacrificed. Let the paper-maker and the undertaker, if they will, enjoy it; but let us not have unsuspecting people poisoned by its continued use."

Case 2.—Cutaneous Affection terminating in Ulceration.—The Rev. J. R., on his appointment to Helmingham, had the parsonage-house put into thorough repair, and painted and papered throughout with various shades of green. Soon after taking up his residence with his family, the children were observed to be covered with a peculiar dusky skin-eruption. On certain parts of the body and about the arm-joints, the eruptive patches spread and ulcerated. The eyelids were also affected, and the disease proved obstinate. Change of air was recommended, and the children were removed, when a rapid improvement took place, and in two or three weeks they were able to return home. In a few days' time, however, the eruption again showed itself, and the skin became very irritable. Attention was then directed to the green paper on the walls of the nursery. This was examined, and found very arsenical. On its removal, the children's health improved, and the eruptive disease entirely disappeared.

Case 3.—Skin-disease: Intermittent Convulsions.—The Rev. R. J. S. went to York, and he and his family took up their residence in the house of the former vicar. In a very short time, Mrs. S. noticed that the children were suffering from a skin-disease. A medical man was called in, but the affection proved intractable. On the occasion of one of his visits, he observed to Mrs. S. that an only child of the Rev. Mr. G., the former occupant of the house, had suffered from a similar eruption, and had had a very narrow escape of its life from intermittent convulsions, and that the child immediately recovered after the family left York. Just about this time, letters appeared in some of the public journals on wall-paper poisoning; and the Rev. Mr. S.'s attention was thereby directed to a brilliant green paper on the nursery walls. This was examined, and pronounced arsenical. The children were at once removed, and the next day he sent for a tradesman to strip off the wall-paper. In the course of the day, both he and his assistants were seized with cramps and sickness, and the reverend gentlemen had to administer sundry doses of brandy and water to keep them at their work. The room was repapered with a non-arsenical paper, and the children subsequently had no return of the skin-affection.

Case 4.—Catarrhal and other alarming symptoms of Arsenical Poisoning.—In March last, Miss S. moved into a furnished house, and in a few days afterwards she was suffering with a cold and cough. Being subject to attacks of bronchitis, she kept her

bedroom, and had a large fire lighted in it. Miss S., however, continued to get worse, and symptoms of a more unpleasant character began to show themselves. She was frequently sick, and suffered from continued pain over the stomach. There was considerable irritation of the throat, with cough and some difficulty of breathing. The general debility increasing, was accompanied by fainting fits, which occurred daily. A friend who called to see her, made a remark on the suspiciously bright green paper on the bedroom wall. She was advised to try the effect of a change to another room. This she accordingly did, by making an exchange with her maid, and every day her health continued to improve. But now another difficulty arose. At the end of a week, her maid was taken ill; a favourite Persian cat, the constant companion of Miss S., was also observed to be covered with a peculiar skin-eruption, and her hair fell off in large quantities. A piece of wall-paper was examined, and found to contain "arsenic in abundance." The wall-paper was removed, and Miss S. and her maid quickly regained their usual health.—*British Medical Journal*, June 14, 1879, p. 891.

85.—ON THE ACTION AND USES OF BELLADONNA.

By J. R. GASQUET, Esq., M.B.

I think all the effects of belladonna may be accounted for by supposing that it paralyses the peripheral nerves belonging to the cerebro-spinal system, except those supplying voluntary muscles; while it has no effect upon the nerves derived from the sympathetic.

I have already mentioned its action upon the sensory nerves, when used hypodermically; in the case of the eye, its paralyzing influence upon involuntary muscular fibre is also exemplified. This is remarkable by the minute amount needed to dilate the pupil ($\frac{1}{130000}$ grain, Donders); the dilatation of the pupil is apparently not influenced by division of the sympathetic, and is therefore due merely to paralysis of the terminal branches of the oculo-motor nerve. On the heart belladonna first has a slackening influence, which may, I suppose, be accounted for by diminished activity of the afferent nerves in the endocardium. But this is slight and transitory, and is immediately followed by acceleration and irregularity of action. This is due to arrest of the controlling influence of the vagus, which loses its inhibitory power in an animal poisoned by atropin; Böhm having shown that this is due to paralysis of its terminal branches. The blood-pressure is not increased, for the vaso-constrictor nerves are at the same time paralysed, and the arterioles dilated, to which the exanthem sometimes observed in belladonna poisoning seems to be due. The muscular fibres in the intestinal

canal, bladder, ureters, and uterus, are also affected; but it has no influence on the voluntary muscles. This remarkable selective affinity is the exact opposite of that belonging to curarin, which completely paralyses all the voluntary muscles, while it has little effect upon unstriated fibre.

The arrested secretion of the salivary and sudoriparous glands is at first sight more difficult to explain, but is apparently due to paralysis of the terminal branches of the cerebro-spinal nerves in the glands. Heidenhain has recently inferred from his experiments that the secretory nerves in the salivary glands are derived from the cerebro-spinal centres, while the sympathetic nerves are simply trophic; and it seems to be established by Vulpian and others that the sweat is secreted under the influence of the cerebro-spinal nerves. It is therefore consistent with what we have otherwise learned to suppose that belladonna paralyses the terminal secretory nerves, and so arrests both sweat and saliva.

I have occupied so much time with this sketch of the action of belladonna that I must pass more briefly over its uses.

1. It relieves pain, sometimes very considerably; and I think it will be found that this is usually of local and peripheral, not central, origin. Thus it is often useful in toothache (less markedly, however, than gelsemin), and in rheumatic or gouty pain; while it has no effect, so far as I know, upon the neuralgias of hysteria or other centric affections. Perhaps its effect upon epilepsy is due to its arresting the centripetal stimulus producing the fit. Whether this be so or not, I am inclined to think it has been too much neglected since the use of the bromides; and that, sometimes at least, it has an effect when these fail; it may also be advantageously combined with them.

2. Belladonna relaxes spasm of the involuntary muscles. Its effects on the eye belong to a subject which is too special for me to handle; its power of dilating the os uteri, and relieving tenesmus of the bladder and rectum when applied locally, is equally important, but perhaps less well known. A further example of this mode of its action is its influence on constipation, which Trousseau first made prominently known. This action upon involuntary muscles is frequently increased by the anæsthetic effect simultaneously exercised by belladonna upon the afferent branches of nerves which originate spasm, as in the cases of whooping-cough and spermatorrhœa; in this latter condition I have found it on the whole more reliable than the bromides.

3. The power of checking sweat is sometimes very valuable. Dr. Fothergill has recently insisted very strongly upon the value of belladonna, as enabling us to arrest the exhausting sweats of phthisis, and I am quite satisfied that, as a rule, this is bene-

ficial. My own very limited experience, however, leads me to doubt whether it is always an unmixed good to check the sweating of phthisical patients, at any rate when the pyrexia is high and the perspiration seems to be of service in reducing temperature.

I have repeatedly tested the efficacy of belladonna in checking the abundant salivation to which some lunatics are subject, and have always found it act with great rapidity. Its arresting the secretion of milk, and the collection of pus in an abscess, are instances of the same kind.

4. I now come to the power which belladonna has of arresting inhibitory action. In medicinal doses it sets the heart free from the controlling action of the vagus, without any other effect upon its innervation. It may, therefore, to some extent replace or assist digitalis, and may be preferred, if we do not desire to act upon the muscular structure of the heart, or to increase the blood-pressure by contracting the arterioles. It seems to be particularly indicated in cases of pure inhibition, and under this head I venture to recommend its trial in the following conditions:—In collapse from shock it seems likely to be very useful, and has been recently strongly recommended by an American physician. It would probably be equally successful when the heart's action fails from sun-stroke, or, on the other hand, from exposure to intense cold. So far as I know, no explanation has yet been suggested for those fatal cases of scarlatina, small-pox, and other infectious diseases in which the patient is, as it were, knocked down at once, and dies speedily, often before the rash has appeared. I cannot go into the reasons which lead me to think that here too we have to do with an instance of collapse from shock; if so, belladonna, or the hypodermic use of atropin, is well worth a trial, and may succeed where all else has failed. Dr. Harley urges its use in the adynamic stage of the infectious fevers, finding it then act as a powerful stimulant; here, also, I suppose it would set the heart's action free. I can less readily understand how it should speedily relieve acute local inflammations, as Dr. Phillips asserts of tonsillitis and meningitis.

There is another important therapeutical property of belladonna, which is probably due to its checking inhibitory action; I mean its power of neutralising some of the effects of opium. Without discussing the whole question of the antagonism of these two drugs, I may mention the following cases in which it is useful:—When added to a hypodermic injection of morphia, it often prevents the nausea and vomiting which this may produce; and when given with an opiate in phthisis, it not merely prevents sweating, but appears to keep up the activity of the lungs and heart.—*Practitioner*, May 1879, p. 326.

86.—ON LOADED AND BLOCKED RECTUM, AND
MANIPULATIONS PER RECTUM.

By Dr. G. De GORREQUER GRIFFITH, Senior Physician to the
Hospital for Women and Children.

Some years ago I was called to attend a child who had been ill for several days, seemed to be in great pain, and was constantly forcing and bearing down. With difficulty I could make an examination per anum; but on doing so with the forefinger, I found a stony-hard, dry mass blocking up (like a ball valve) the entire rectum. I could not break it up with the finger, and had to content myself with giving aperients with the object of inducing watery evacuations that would empty the upper bowel, and also fine down and soften the hard fæcal mass below. The medicines happily had the desired effects, and on my calling next day I was just in time to find my little patient in, as it were, the forcing throes of labour, from which she was soon delivered by the expulsion of a mass quite as large as a good sized William pear. After this evacuation the general symptoms of malaise speedily disappeared without any trouble.

In April, 1879, a lady about fifty-five years of age came to me with symptoms of blood-poisoning, and of being generally out of health. She was relieved of these symptoms; and shortly afterwards she sent to me saying she was so ill from piles, that she would be unable to call upon me. One of my assistants, Mr. Bamber, accordingly visited her; but as she declined to allow an examination, he prescribed some aperient medicine to be taken and a soothing ointment to be applied to the anus. Next morning she drove up to me, and on passing the forefinger into the rectum, I found two hard fæcal masses in the bowel, ovoid in shape, stone-like in hardness; one being about the size of a large William pear, and the other of a smaller; they were both movable, the mucous lining of the intestine being not dry, but well lubricated with an excess of the natural secretion, the calibre being large. Tender, and hyperæsthetic, as were the anus and all the adjoining parts, dense and unyielding as were the ball-valve-like obstructions, I knew it would be useless to attempt getting them away by either the fingers or the hand introduced in recto, or by scooping them out with spoon or otherwise. I therefore ordered her to take by the mouth a jalap and sulphate of soda aperient, accompanied with calomel, and by the rectum to have an injection of warm water administered, for the purpose of softening and melting down the scybalous masses, of irritating to action the lower bowel, and thus expediting their removal. Two days afterwards she came to me; and it occurred to me, that instead of giving her chloroform, as I had intended, and introducing the fingers, or the

entire hand, or some scooping instrument, I would see what could be done by passing the fingers of the right hand as high up as possible into the posterior *cul de sac* of the vagina and against its posterior wall, thence exercising backward and downward pressure upon the masses, while the fingers and thumb of the left hand, buried in the ischio-rectal spaces (devoid of fat in the present instance) compressed the bowel, and the therein contained *fæces*.

The aperients had operated well; the masses were reduced in size, and somewhat softened, and the consentaneous double-handed expedient which I adopted for their removal was so effectual, that they were far more readily forced away than I had anticipated, and without the use of any anæsthetic. True, there was of necessity a good deal of dilatation of the lower part of the bowel, and the mucous lining of the anus was split in some places; but there was very little suffering, no faintness, no exhaustion, and the patient was able to drive home very shortly afterwards.

While on the subject of emptying the rectum, I may be pardoned for quoting the words of my paper "On Post-Partum Hemorrhage, and Modes of Controlling and Arresting it," published in the *Obstetrical Journal* (Feb. 1879, page 707).

"I believe we do not at all sufficiently utilise the rectum as a way of usefulness, and an open road of power; a way of which powerful advantage may be taken—a pathway always ready to our hands, for diagnosis and prognosis; of remedial measures, and often of curative: a pathway, I consider, too frequently forgotten, neglected, or considered not worth using, both by physicians in medical cases, by surgeons in surgical, and by gynecologists and obstetricians in their particular branches. In this opinion I am well sustained by Mr. Spencer Wells, by Dr. Priestley, Dr. Broadbent, Mr. Maunder, Mr. Teevan, and by the late Professor Simon of Heidelberg."

In a paper which I read before the Harveian Society, and which was published in the *Edinburgh Medical Journal*—"On Fæcal Accumulations Stimulating Utero-Ovarian Tumours," I state, "I am the more encouraged to bring these cases forward, because you, Sir—our President—and Dr. Broadbent, and Mr Teevan agree in the value of free exploration of the bowel" (in the manner to which I refer). I have no hesitation in recommending the introduction of the hand and arm for this purpose, provided of course that there are not contra-indications, and that the operator's hand and arm are not large, are rather small than otherwise, are not in unsafe disproportion to the dimensions of the rectum.

I do not know in what year Professor Simon first advocated and adopted the exploration of the interior of the abdomen by

means of the hand and arm passed into the rectum ; but I am fully aware that he laid great stress on the advantage of such a procedure, as an aid to diagnosis. A most distinguished surgeon of our own city (London), Mr. Maunder, adverted to the plan many years ago, and actually put it into practice in 1868, as he personally informed me, and as we also learn by his remarks on "Diagnosis by the Hand in the Rectum." (See *Lancet*, 1868, p. 586, and vol. 1,469.)

As regards the exploration of the bowels by means of the hand and arm introduced per anum, it is hardly necessary to say that, according to the size of the operator's hand, and that of the patient's rectum, there is a correlative forcible distension, and corresponding rupturing in the mucous membrane of the anus ; and, in some cases, of the sphincter ani also ; indeed, sometimes this latter may have its fibres stretched, and even torn to such an extent that incontinence of fæces results, but only for a short time, the mischief being quickly repaired—eight or ten days sufficing to remove the inconvenience by setting the parts right again. I have never known violent inflammation of the bowel result from this procedure, though inflamed piles have followed as a direct sequence.

Of course, if there were any suspicion of malignant disease affecting the bowel, the hand should not be forced through the anus : but that would have been previously ascertained, as would also the general conditions of the rectum, by an examination conducted with the forefinger. These are precautions which immediately suggest themselves, and hardly require to be mentioned ; yet, as it is well to bear them in mind, I shall not withhold the warning. To arrest post-partum hemorrhage, we may introduce the *left* hand into the rectum, passing it as high up as may be needful to command the entire uterus, and to employ an effective force, directed from above downwards, while the *right* hand, placed externally on the abdomen, compresses the womb from above downwards and backwards, the two hands, thus acting consentaneously on the organ, encompass and compress it between their simultaneous grasp, thereby arresting the blood flow, and at the same time stimulating the uterine muscular coat to natural contractions ; thus in two ways preventing dilatation of the womb.

In parenthesis I would here say that by means of the hand in the rectum we have a ready method of completing enucleation and expulsion of intra-uterine growths, when their ablation by that method is considered preferable, and after their outer coats, or shell, so to speak, have been laid open by incision or by some other method of effecting a breach of continuity in their surface.

As bearing on the subject of "the arrest of post-partum

hemorrhage by means of the hand in the rectum," I will here append a recent letter from me to the Medical Press and Circular, in which I referred to the not unfriendly criticisms made, in that periodical, on my paper which appeared in the Obstetrical Journal for February, 1879. The words of the kindly reviewer are:—

"But it has been recently shown that introduction of the hand into the rectum is not only a difficult operation, but one that is attended with some danger. Besides, the safe introduction of the whole hand into the rectum should be effected in a gradual manner, and while the patient is under chloroform; whereas, its introduction for the purpose of compressing the aorta in cases of post-partum hemorrhage would have to be immediate, and without the assistance of anæsthetics."

It is quite true that the introduction of the hand into the rectum is a difficult operation, and attended with some danger, if the operator have a large hand, and be rough and hurried in his manipulations, thrusting and forcing the hand into the bowel, or attempting to do so, instead of gently, and with boring motion, working in the fingers folded together cone-shaped, and well larded, and then the hand, narrowed as much as the operator possibly can. If there were malignant disease of the bowel, or organic stricture, then, indeed, if the operation were attempted, it would be difficult and most dangerous. But in such instances no man should be rash enough to attempt such a proceeding, and would, of course, by digital examination, first ascertain the state of the bowel. I can understand that in some cases other than child-bed, it would be especially "difficult," if called upon to perform it without being able to have had the bowels cleared out, though such must be extremely rare; and "dangerous" to some, because of pain and shock, if anæsthetics could not be administered. But in the post-partum patients, to whom I refer, it is not so difficult, because the bowels have been unloaded during the parturient efforts; their muscular coats are relaxed, as well as the sphincters, owing to the post-partum loss and hemorrhage, which call for such interference; and the necessity for inducing anæsthesia obviated because of the unconscious or semi-unconscious state of those patients in whom the operation is needed; and the introduction of the hand will, indeed, oftentimes be a judicious stimulus towards rousing the sinking energies.

It must be borne in mind that it is a last resort; and it will prove a very effectual one, I have no doubt; that the patient is *in extremis*; and that there is only the choice between her dying of hemorrhage, and the performing of this operation, which the reviewer speaks of as "difficult, and one attended with some danger."

The difficulties, I hold, are in a great measure removed by the carefulness and skill of the operator, provided he has not an unduly large hand; the danger also is reduced to a minimum, for the same reason, as well as because of the prostrate and lethal, or lethargic state of the patient. Moreover, I greatly question whether, in such a post-partum condition, there is more danger (or even as much) in introducing the hand into the rectum, than into the vagina and uterus; and whether there is more risk in this procedure, than in injecting iron into the womb—flabby, relaxed, and unable, through general and local debility, to contract in response even to such stimulation, and when the hand within its cavity has failed to excite to natural contractile efforts. By tourniqueting the aorta with the back of the hand which is in recto and is compressing the uterus—in the manner I have named—we have time to have stimulants given our patient by mouth and nose; we are enabled to have the womb also compressed by the hands of an assistant; to permit it to rest; to allow clotting of the blood in the open uterine vessels, the rush of blood being stopped or very greatly eased by the compressing; while, at the same time, we throw back upon the vital organs the blood, diminished in quantity, and keep up their supply, thus sustaining them, and preventing the stoppage of their functions and the cessation of life.—*Practitioner*, July 1879, p. 28.

87.—ON IODOFORM.

By Dr. ALFRED SHEEN, Surgeon to the Glamorganshire and Monmouthshire Infirmary.

Iodoform is a bright yellow crystalline body with an extremely pungent odour. Chemically it much resembles chloroform, except that the three atoms of chlorine are replaced by three of iodine.

The use of this substance was advocated by Mr. Berkeley Hill in a short paper which appeared in the *British Medical Journal* on January 26, 1878; and in his *Student's Manual of Venereal Diseases*, published recently, he says: "All chancres are best treated with iodoform: under its use healthy sores heal rapidly, creeping sores generally cease to spread, and sluggish ones take on healthy action." My own experience supports this statement most fully. I cannot explain the manner in which it acts, but that it does have a most remarkable effect in promoting the healing, not only of ordinary chancres, but of many other sores, I can have no possible reasonable doubt. It is what I might call a *reliable* remedy, and often saves one a deal of trouble. Its effects seem almost magical. You sprinkle a little of the crystals, powdered or unpowdered, over the sore,

cover this with a bit of dry lint, or vaseline spread on lint, and at your inspection next day you find that healing has progressed rapidly, the sore has filled in considerably if it is a deep one, there is but little discharge and no smell, and you have only to repeat the dressing, and so go on from day to day until healing is complete. The relief from pain is often remarkable. In illustration of these remarks I will give a few cases from my private case papers.

Case 1.—Mr. R. consulted me on January 18, 1878, suffering from a chancre, involving the end of the urethra and the glans for the distance of about $\frac{1}{6}$ th of an inch. He lived twenty miles away, came to see me at irregular intervals, and said he could not give up his work as an accountant. The sore got rapidly worse, and eventually, on my advice, he took lodgings in town where I could see him daily. On February 12th I had him well in hand. He was sent to bed, and as he had a roseolous rash and sore throat I put him on gr. 3 pil. hydrarg. daily, which he took till March 9th. The only local treatment he had was iodoform, applied in the way I have indicated, once or twice daily. The sore at once commenced to heal, and he was sent home on March 19th quite well. He suffered from no subjective symptoms whatever during the treatment, except when, on February 26th, black wash was substituted for the iodoform by way of experiment, which was followed by so much discharge and pain that the iodoform was again reverted to and continued to the end.

Case 2.—Mr. B. consulted me, suffering from a chancre about the size of a threepenny bit on the glans. I gave him a small quantity of iodoform powder to be used morning and evening. He did not call again, but a short while after I met him accidentally, and on asking after his sore he said, "Oh, it healed up before I had used half the powder." (This is one of the disadvantages of the remedy to the general practitioner, but of course in our noble profession we take no account of that!)

Case 3.—Mr. T., a cabman, sent for me on June 15th. He was suffering from a punched-out looking sore on his leg a few inches above the ankle, and over the seat of a former compound fracture. He had great pain; there was much swelling, with bluish-red shiny surrounding skin, and altogether matters looked very unpleasant, especially as he had a new situation that he was to go to on the 17th. I managed to keep him in bed, however, till the morning of the 19th, and by the aid of rest and the constant application of Goulard's lotion got the swelling and inflammation subdued. On the morning of the 19th I sprinkled iodoform into the sore, covered it with a bit of dry lint, applied a bandage from the toes to the knee, and he went to his work. I was certainly curious to see how such an

apparently unfavourable case would go on. On the 20th the dressing was reapplied; there were no subjective symptoms, the pain had been at once relieved and had not recurred, and the sore had commenced to heal; the lint was stained brownish-red, but there was no discharge. From accidental circumstances the dressing was not disturbed till the 23rd (three days), and I felt anxious about the state of the ulcer and surrounding parts, but on removing the bandage and lint I was pleased to find everything *in statu quo*, except that the ulcer had filled in considerably. The iodoform was then re-applied about every third day, and by July 10th the ulcer had healed soundly.

Case 4.—J. W., æt. 18, consulted me on October 26th. He had had a chancre, followed by buboes above and below Poupart's ligament on the left side, attended with great pain. These buboes had suppurated, and were rapidly extending from inflammation and want of rest. On the 29th there was much sloughy discharge and great pain, and surrounding redness of skin, a large gland lying isolated in the upper part, and the whole divided into two parts by a mere strip of skin, $\frac{1}{4}$ inch wide, in a line with Poupart's ligament. He was put to bed, Goulard's lotion, with hourly syringing with warm water, were successfully used to reduce the inflammation, and on November 2nd, when the use of iodoform was commenced, the upper part of the sore measured $1\frac{1}{2} \times 1$ in., the lower, $2\frac{3}{4}$ in. \times 2 in., and was very deep. The effects of the iodoform were simply marvellous. All pains ceased and never recurred, the sore commenced to heal, assuming a bright red healthy appearance, and the discharge was reduced to a mere nothing, and altered in character, looking like serum and having no odour. The whole sore was soundly healed by November 30th.

At our Union Hospital iodoform has with me been the routine treatment of chancres and some other sores, and with very satisfactory results.

Any who may feel further interested in this subject I would refer to the British Medical Journal, vol. i., 1878, pp. 20, 127, 190, 193, 294, 367, and 710; Practitioner, vol. iv. p. 308, and vol. vi. p. 380, and to a paper by Dr. Moleschott of Turin, in the Medical Record of November, 1878. The latter speaks of its value in getting rid of glandular swellings, even such as occur in lymphoma and in splenic leukæmia, in orchitis, in effusion into serous cavities, in acute hydrocephalus, in chronic arthritis, in intercostal and other neuralgias, &c. He says that before promoting absorption it determines the destruction of the primitive elements, and that its power may be regarded as miraculous. He uses as an external application 1 to 15 parts of elastic collodion, or 1 to 15 parts of ointment, and advises that these preparations should be kept at a distance from the

window, in a well-closed tin case, thus retarding the decomposition of the iodoform, which goes on rapidly in the light. When used internally he gives $\frac{3}{4}$ gr. to $1\frac{1}{2}$ gr. daily in pills. He winds up his paper by saying, "That, notwithstanding its inconveniences, I dare promise for this remedy a great future." Its inconveniences are its pungent odour and its liability occasionally to cause eructations and palpitation of the heart.—*Practitioner*, May 1879, p. 321.

88.—ON AN IMPROVED APPARATUS FOR SPRAY INHALATIONS.

By Dr. JAMES ADAMS, Glasgow.

It is now ten years since I read before the Glasgow Medico-Chirurgical Society, a communication on "Medicinal Inhalations, with description of an improved apparatus for the production of medicated vapours." On that occasion I exhibited an Inhaler which was cordially approved by all the members who made it the subject of comment. That approval gave me gratification that compensated for an expenditure of much time, thought, and pecuniary outlay. The article immediately came into very general use. It was at first sold under the name of "The Adams Inhaler," but of late years, it was pirated, and has since been sold under the name of "Dr. Siegle's Patent Steam Spray Inhaler, with Boiler as suggested by Dr. Adams."

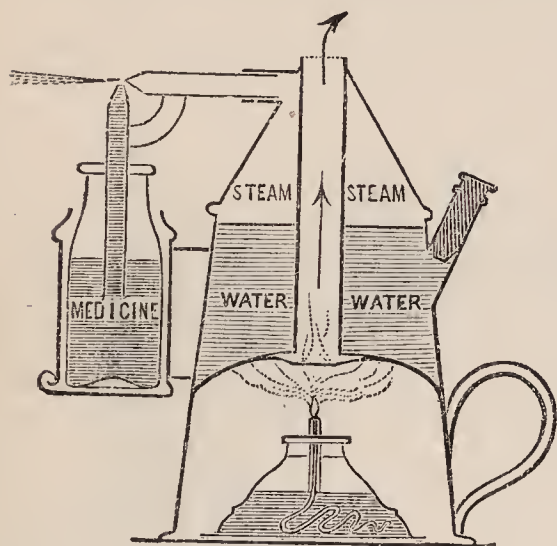
In Siegle's *Patent* Inhaler, the form of boiler is spherical, with a flat bottom. It is that known as the ordinary land boiler, consisting of one large mass with a great quantity of water in the centre. The heat is applied exclusively to the bottom, with the result that the water is evaporated at a low temperature, and the steam that is formed has the least degree of elasticity or force. It is called a low pressure boiler. The steam being produced at a slow rate of development, the medicated liquids cannot possibly be pulverized or rarefied so minutely, projected so forcibly, or the spray maintained so uniformly as when steam is used that is formed in a high pressure boiler. When there is—as in the present case—no water line or other gauge for regulating the quantity of water originally placed in the boiler, and for preventing overfilling, there is great liability to several serious accidents. Thus, as explained by Donnè, when water has been deprived by heat of that portion of air which it normally contains, there then occurs a capricious variability in its capacity of expanding and of generating steam. In practice, this annoying and dangerous peculiarity cannot be readily regulated, and hence a vessel of boiling water will now and again boil jerkingly and boil over; hence also a safety valve will frequently be seen in action, fitfully and intermittently. For

want of a water line, the boiler in Siegle's Inhaler occasionally gets overfilled, and—owing to the cause specified—it boils over, or “primes,” as it is technically called, and the water flowing over through the steam escape is projected forcibly in the face of the patient. It was from witnessing this accident frequently, that I was first led to make efforts in the way of improvement, for I found that it required much persuasion on the part of the physician, and considerable nerve on the part of the patient, to face Siegle's *Patent Inhaler* after one or two experiences of this nature. The liability to accidents through scalding was further increased by the angular shape and great length—4½ inches—of the steam escape pipe which rose vertically from the top of the boiler, and was bent at an acute angle in a horizontal direction. The low pressure steam, as it passed [sluggishly through this long angular pipe, got cooled and condensed, and the drops collecting at the angle were forcibly spirted in the face of the patient. The faulty *shape* of the *patent* boiler was aggravated in the attendant results by the faulty *material* of which it was composed, *i.e.*, *glass*. For when water is boiled in a close, or nearly close vessel, the quantity of steam generated is often suddenly doubled, and consequently the capacity of the containing vessel is suddenly subject to a double strain. The choice of a *glass boiler, with a securely fastened stopper*, was therefore a very faulty and ignorant choice, for glass is an imperfect conductor of heat, and cannot withstand sudden expansion or contraction. Hence the explosions which, singularly enough, were anticipated, and which actually occurred, notwithstanding the assurance of the maker (F. Mollenkopf, of Stuttgart), that each glass boiler was carefully tested under a pressure of two atmospheres, or four times greater than, in his opinion, was necessary; for the force of steam required for pulverizing fluids. The suddenness of the expansions and contractions I have referred to had not been taken into account, or were insufficiently understood and appreciated.

I need not continue this depreciatory criticism of the many practical defects I might enumerate in the fabricating of this crudely fashioned *patent* instrument.

I will now refer to some of the merits which I claim for the model constructed by myself. *First*, it is entirely made of tin, and therefore strong; it is very portable and compact, and therefore easily handled and not readily disarranged. *Second*, it is sold by the retailer at 5s., a price that, while a mere fraction of the charge formerly exacted, is within fair limits of a physician's prescription and of an ordinary patient's means. *Third*, the boiler is markedly distinctive, and has many advantages. It is of tubular construction, and its expanded bottom and central flue present a large surface for the application of heat, the rapid

and plentiful generating of steam, and—what is of even greater moment—the drying, or rarefying, or superheating of the steam after it is generated. For the central flue, which passes through the boiler and provides a chimney to the spirit lamp, also carries the flame with a sharp draught, and consequently with an intensified heating power through the steam chamber. The steam collecting in this chamber is, at the instant of escape, brought by the conical-shaped top into close contact with the central flue, and is thus superheated and dried, and delivered in a more elastic condition, thereby ensuring a more effective pulverization of the medicated fluid. By carrying the flue of the lamp through the centre of the boiler, excessive heat is diverted from the outside where such heat would be inconvenient,



Sectional View.

and therefore, a handle which is attached to the lower and cooler part of the instrument can be grasped with comfort and safety, even when in use. The position of the water inlet at the side of the boiler, instead of the top, prevents overfilling, and at same time provides a water line and a defined steam reservoir. *Fourth*, a common cork acts as stopper to the boiler, and is a perfectly sufficient substitute for the alarming looking and costly safety

valve that Siegle had adopted after suggestion from without. *Fifth*, the steam escape pipe is only 1 inch long, has no angles—consequently does not cool or condense the steam—and scalding accidents never occur.

There are other details of minor importance which in their combination make the apparatus more convenient in use.

And now I come to the more novel and practical portion of my communication. A very annoying incident has hitherto attached to the use of the spray inhaler. The spray scatters over the face, eyes, and dress of the patient, and of his bed clothes when in bed. A separate apparatus, consisting of a screen with a hole in the centre, mounted on a support, and having a vessel to contain the condensed spray as it trickles down, has been recommended, and is *occasionally* employed; and a funnel shaped screen of card board, or of stout paper, or other material held in the hand, is *frequently* employed. But in practice the use of even the best of these make-shift contrivances, setting aside the extra cost, is insufficient to prevent annoyance. There is difficulty in adjusting the stand-mounted screen so as

to save the patient from a strained position, particularly while in bed. The vapour, moreover, does not always get through the hole in the screen, but is caught at an angle and deflected with every movement of the hand or face. There is a further annoyance caused by the vapour that is intercepted by a hand screen becoming condensed on the screen, and from thence trickling down upon the bedding, or upon the table, if the patient is out of bed. I have found these inconveniences to be substantial drawbacks to comfort and efficiency in the use of spray inhalers, and I have devised a remedy. This consists in a



Dr. Adams' Inhaler with "Face Protector."

light tin funnel about 5 inches long, 3 inches wide at one end, and three-fourths of an inch at the other. A narrow slip of tin soldered to the wide end of the funnel, and sliding into a groove on the body of the inhaler in front, places and keeps the funnel in a horizontal position. This addition to the inhaler, which I will call the "face protector," conducts the

spray projected through the wide end of the funnel, while the narrow end is placed within, or close to, the patient's mouth. The wide end of the funnel is partially enclosed at its lower edge for about a fifth part of its diameter, thereby forming a cup or receptacle for collecting the spray that condenses within the funnel, and which would otherwise have been dissipated and lost. The protection afforded by this little arrangement is so complete, and its other advantages so decided, that I cannot doubt it will be found attached to the next edition of "Siegle's Patent." The apparatus is subject to no disarrangement in whatever position it is handled, and the vapour is directed fully and fairly into the mouth or nostril of the patient as may be desired. The face protector is attached or detached with instant facility, so that it is no obstacle when the physician desires to avoid a concentrated medicinal inhalation, as in the case of infants, or of sleeping invalids. Besides protection to the face, it acts as a "spray economizer," and that in two modes. Through experiment I have found that a larger proportion of the medicinal vapour is conducted into the mouth of the patient, and actually inhaled during its use. Further, that portion of the spray which, under previous conditions became dissipated and lost, is now collected, and may be used to dilute subsequent inhalations. The condensed and collected fluid has, of course, become weaker through dilution with watery steam. But the extent of this dilution is readily ascertained, and, on an average, the collected fluid may be taken as half the original strength. Knowing this fact the patient may regulate the after dilution of his medicine, using the collected spray instead of plain water. I am assuming that inhalation medicines are ordered in a concentrated form, by other physicians as by myself, out of consideration for the pocket of the patient.

To my thinking, this little appendage to my inhaler will prove so useful that it will henceforward be considered essential to the completed instrument. I am gratified to learn from the manufacturer that the actual manufacturing cost of the addition is so trifling, on account of the large scale on which the instrument is being made, that he means to make no addition to the selling price.—*Glasgow Medical Journal*, March 1879, p. 182.

89.—ON SOME POINTS IN MATERIA MEDICA AND THERAPEUTICS.

By Dr. WALTER G. SMITH, Assistant Physician to the Adelaide Hospital, Dublin.

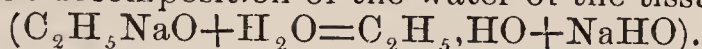
Vaseline.—This substance, which has justly gained a widespread popularity with English practitioners since its introduction from America some two or three years since, has not

failed to attract attention in France, and we glean from the *Répert. de Pharm.* a few additional particulars to those given in the Report for September 1876. When crude petroleum is distilled, after the separation of the light oils, there remains in the apparatus a semi-liquid tar, which constitutes crude vaselin. In this state it has a disagreeable odour which discloses its origin; it is black, and has a strong taste. The tarry compound is heated in the open air, and then decolorised by animal charcoal, the product being commercial vaselin, which is a mixture of several hydrocarbons. When pure, vaselin is white, inodorous, and insipid. Its consistence is that of jelly, or of a very unctuous fat. It melts at 35° C., boils at 150° , distils at 200° , and burns without residue. A circumstance worth noting is that under the prolonged action of light it acquires a slight odour of petroleum. Insoluble in water, and sparingly soluble in alcohol, it dissolves freely in warm ether. It is very soluble in fats, volatile oils, chloroform, and sulphide of carbon. On its own part it sensibly dissolves iodine, bromine, sulphur, phosphorus, carbolic and benzoic acids, and almost all the alkaloids—especially strychnia and atropia. Its composition indicates that it is capable neither of becoming rancid nor of being saponified. Hence it may be utilised as an excipient for caustic alkalies, oxides, and metallic salts, and even for acids, in the cold, without being attacked by them, and without modifying their therapeutic properties. In various complex and easily alterable ointments the introduction of a certain quantity of vaselin will insure preservation and hinder rancidity. Dr. Galezowski, as the result of numerous experiments, affirms that vaselin is unquestionably superior to any other excipient hitherto used in the preparation of ointments—such as butter, lard, glycerin, and glyceroles.

Sodium Ethylate, or Caustic Alcohol.—At the meeting of the British Association for the advancement of Science, held at Liverpool in 1870, Dr. B. W. Richardson brought the ethylates of sodium and potassium, with some other of the ethylate series, into notice for practical use. In the same year he removed, by the use of sodium ethylate, a large nævus from a child who was under the care of Mr. Gay. The nævus had previously been subjected to treatment by the ligature and by other methods, including the free use of nitric acid, without success. In the course of the next year Dr. Brunton employed the ethylate for nævus with equal success.

Attention has recently been again called to these interesting compounds, and Dr. Richardson has accordingly contributed to the *Pharm. Journ.* a few practical remarks upon the use of the sodium alcohol (C_2H_5NaO), which we reproduce. From Dr.

Richardson's researches it appears that the ethylates of potassium and sodium are bodies which, on being brought into contact with the moist living tissues, are decomposed—caustic alkali being produced and ethylic alcohol being reproduced by the extraction and decomposition of the water of the tissues.



Therefore four results ought to follow the application of an ethylate to a vascular living tissue—viz. (a) a removal or absorption of water from the tissue into the ethylate; (b) the destructive action of a caustic from the caustic soda that would be formed; (c) coagulation from the alcohol that would be reproduced; (d) prevention of decomposition of the dead organic substance that would be formed.

Experience has shown that these results are attained; but if the ethylate be too concentrated the caustic action is over severe, and hemorrhage may follow like as from an incised wound. This is specially the fact with ethylate of potassium, and hence Dr. Richardson prefers to recommend the ethylate of sodium of a strength to be presently named.

In order that the intention of the ethylates may be properly fulfilled, they must be sent out by the pharmacist as absolute alcohols, and for that reason absolute ethylic alcohol must be used in their manufacture. The sodium ethylate is best dispensed in a bottle furnished with a glass stopper, ending in a pointed glass rod which descends into the fluid. The liquid may also be applied very neatly by means of a clean quill, cut like a pen, and newly nibbed each time when used. The glass brush is very inadvisable. The small fibres break off, and, in one instance, a portion of fibre left on a surface touched with the ethylate caused great pain and trouble.

The formula for the ethylate is as follows:—Put half a fluid ounce of absolute alcohol (sp. gr. 0.795) into a two-ounce test tube. Set the test tube up in a bath of water at 50° F., and add, in small pieces at a time, cuttings of pure metallic sodium. A gas (hydrogen) will at once escape. Add the sodium until the gas ceases to escape, then raise the temperature of the water in the bath to 100° F., and add a little more sodium. When the gas again ceases to escape stop adding sodium; or if the fluid, which by this time will be of a gelatinous consistency, should crystallise, then stop. Afterwards cool down to 50° F., and add half a fluid ounce more of absolute alcohol.

Thymol.—Dr. C. Symes has made a few experiments upon thymol which are of interest, as this substance continues to be largely used. They supplement Mr. Gerrard's careful paper on the pharmacy of thymol, epitomised in the Report for July 1878. It occurred to Dr. Symes to try if thymol and chloral hydrate,

when rubbed together in a mortar, would produce a liquid similar to the well-known chloral-camphor. Experiment proved, however that such is not the case; but if an equal quantity of camphor be added to the mixture, the whole at once liquefies and produces a powerful antiseptic. Two drops of this thymol mixture added to a fluid ounce of urine containing pus, which was already in an incipient state of decomposition, at once, and for a considerable time, arrested putrefaction. Further experiments showed that thymol and camphor when rubbed together, in the absence of chloral hydrate, also become liquid, and that the proportions could be varied from 2 parts thymol and 1 of camphor to 1 part of the former and 10 of the latter, the result being a colourless syrupy liquid; equal parts of each gave very satisfactory results. The solubility of thymol in water is not greatly increased by this combination, but it is a very convenient form from which to prepare the ointment. Thymol-camphor can be mixed with vaselin, unguentum petrolei, or ozokerin, in almost any proportion; and an ointment prepared with 20 per cent. of the mixture, equal to 10 per cent. thymol, has been kept for some weeks without any separation whatever. A saturated solution of thymol in water, (1 in 1,000) is found to be sufficiently strong for the spray during surgical operations, but for the throat and various other purposes it is often required stronger, and in such cases Dr. Symes knows of no better aqueous solvent than milk, which takes it up readily in almost any proportion up to nearly 10 per cent. of its weight; but it will be rarely required of such strength. Solution of borax is not a good solvent, but glacial acetic acid dissolves it most readily—a large proportion, however, separates on dilution. The acidum aceticum of the Pharmacopœia dissolves two grains in the fluid ounce. There appears to be some difference in the sp. gr. of thymol, arising probably from the source from whence derived; that described by Mr. Gerrard had a sp. gr. 1.028, hence was heavier than water, whilst the specimens Dr. Symes has met with have only a sp. gr. of 0.980 to 0.990, and float on or near the surface.—(Pharm. Journ.) The powerful antiseptic action of thymol, exceeding under some conditions that of carbolic acid, its small activity as a poison—about one-tenth of that of carbolic acid—and the absence of irritating effect when it is applied to the skin, all point to its use as a substitute for carbolic acid in the now well-known antiseptic treatment of surgical cases elaborated by Professor Lister. This substitution has been made with great success by Professor Volkmann, of Halle. For the spray solution, this surgeon uses a mixture of 1 part thymol, 10 alcohol, 20 glycerin, 1,000 water; but a solution in water only, which will not deposit, may be made by adding 1 part of thymol to 1,000 of hot water. For the gauze dressings used by Pro-

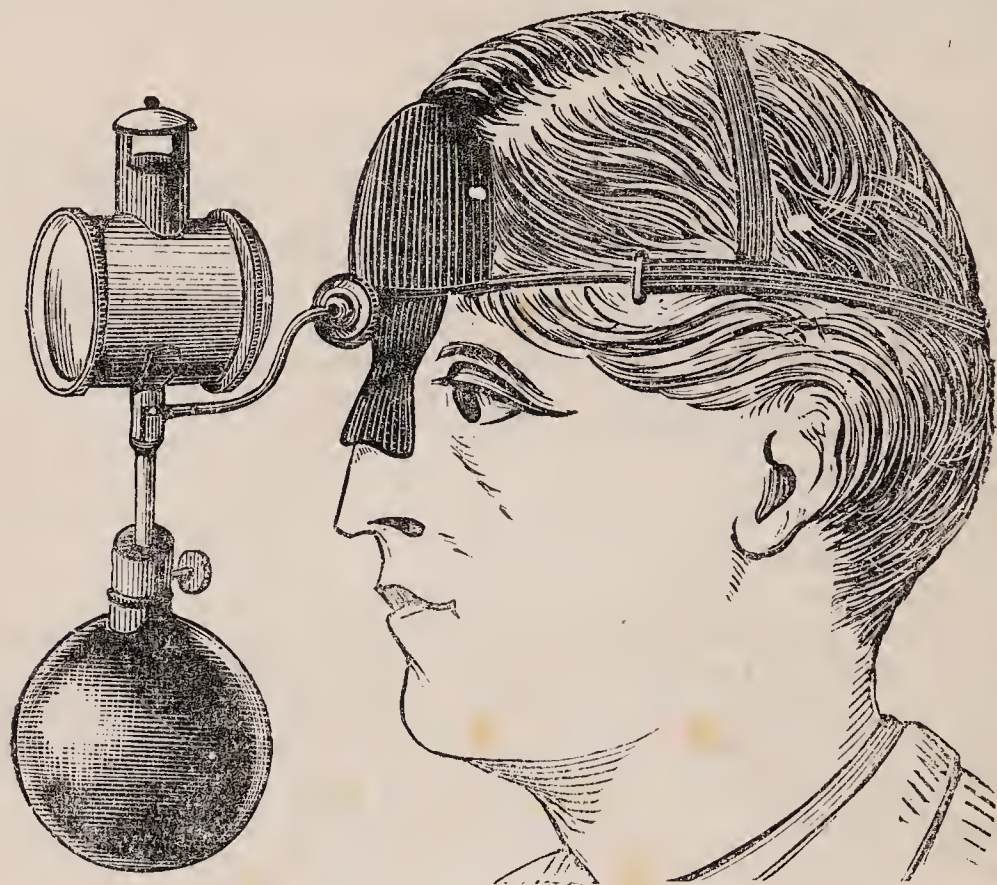
fessor Lister, others were substituted, made by saturating 1,000 parts of bleached gauze with a mixture of 500 parts spermaceti, 50 resin, and 16 of thymol. This prepared gauze is extremely soft and pliant, and sucks up blood and the secretions of the wound like a sponge. The fibres of the gauze being impregnated with spermaceti, cannot of course become saturated with the secretions, so that they do not become stiff. The present cost of thymol is about 5 times the cost of the best carbolic acid, but as one part of the former seems to do as much work as 25 parts of the latter, the advantage of price is on the side of thymol.—(Chemist and Druggist.)

Calomel and Sugar.—In two places attention has been drawn to the danger of keeping calomel mixed with sugar. Dr. Polk has observed a case of poisoning, presenting all the characters of corrosive sublimate poisoning, after the use of a powder composed of calomel and sugar, prepared a month previously, and in which chemical analyses revealed a notable quantity of sublimate. At Turin an analogous case of poisoning has been observed after the use of calomel pastilles. In each of these cases the question naturally arises—Did the calomel originally contain corrosive sublimate, or was there an after-formation of sublimate?—(Rep. de Pharm., Oct., 1878, from Pharm. Zeitsch. f. Russl.) Instigated by the reports of such cases, Herr Slop was induced to make some experiments as to the conditions under which the mercurous is converted into the mercuric chloride, and its probable behaviour towards the juices of the stomach. The general conclusion arrived at was that mercuric chloride was formed in the presence of hydrochloric acid, organic acids, chlorides of the alkalies, and carbonates of the alkaline earths, the last being specially important, considering the frequency with which calomel is dispensed with magnesia. Turning his attention to sugar, Herr Slop examined several specimens of calomel pastilles, having sugar for their basis, that had been made more than a month. Some of these contained a considerable quantity of mercuric chloride, whilst others did not contain a trace. This difference he attributes to the latter having been prepared from a neutral refined sugar, while for the former probably a first crystallisation of beet sugar, still contaminated with the calcium hydrate introduced in the neutralisation and clearing of the juice, or a raw colonial sugar, which, as a rule, would be acid, had been used.

Mr. Langbeck doubts whether sugar perfectly dried and mixed, with well-washed calomel, and kept in a stoppered, coloured bottle, would change into glucose, and thus act as a reducing agent on the calomel, and some experiments which he has made confirm him in this opinion.—(Pharm. Jour.)—*Dublin Journal of Medical Science*, March 1879, p. 229.

90.—NEW FRONTAL ILLUMINATING LAMP FOR MEDICAL PURPOSES.

The new Frontal Lamp, of which an illustration is annexed, was contrived by Messrs. Salt and Son, of Birmingham, at the instance of Dr. Lycett, of Wolverhampton. Its object is to enable the Practitioner, more especially the Obstetric Physician, to illuminate the vagina or rectum without the aid of an assistant, and at the same to leave both hands free for any operative

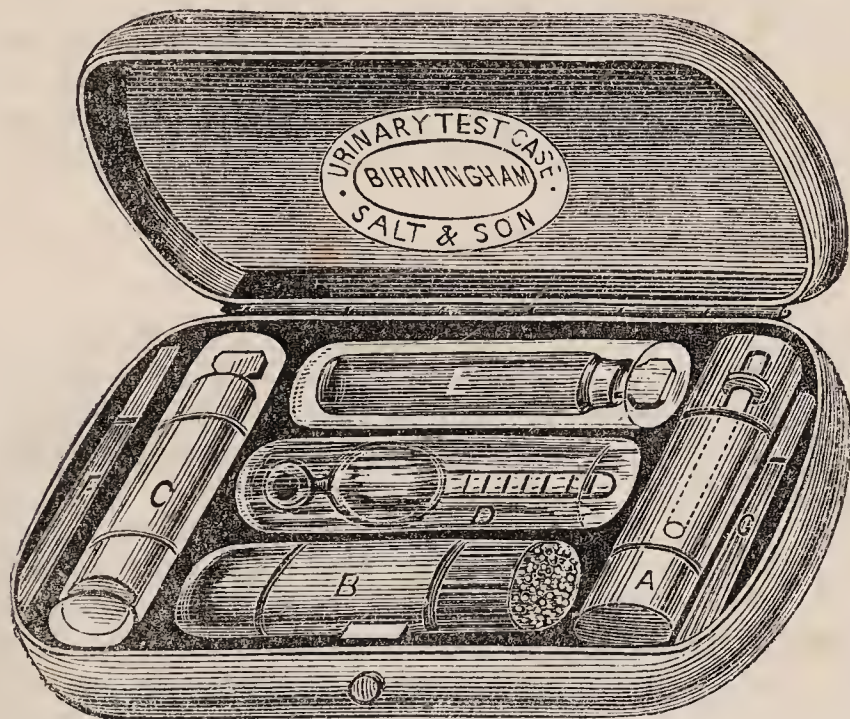


procedure. This is accomplished by securing the lamp, which is very light, to the observer's forehead in such manner that it shall accommodate itself to all his movements, and automatically direct its rays in whatever direction he may be looking. The lamp is fed by benzoline absorbed by a sponge contained in a small receptacle, and will burn for half an hour. As no free spirit is employed all risk of accident is avoided, and a shield of cork protects the forehead from heat. The lamp may be carried with its reservoir filled and ready for use.

91.—SALT & SON'S NEW POCKET URINARY TEST CASE.

This case, which has been submitted to us by the makers, Messrs. Salt and Son, Birmingham, we consider a great im-

provement; it is elegant in appearance, convenient in size, and contains, as will be seen, all that is necessary for ordinary clinical



work. The case is made of aluminium, opening like a portmonnaie, and is divided into compartments for the following articles, viz., Spirit lamp,—Nitric acid tubes hermetically sealed and in a gilt case,—Caustic potash pilules in vulcanite bottle, inclosed in gilt case, each pilule being one test,—Urinometer and test-glass,—Test-tube, and vulcanite stoppered bottle for acetic acid or Fehling's solution,—Turmeric and Litmus paper.

The various appliances are conveniently arranged, and occupy but little space, and the complete case seems likely to be found extremely useful. The illustration is drawn to a scale of two-thirds size.—*British Medical Journal*, July 26, 1879.

92.—IMPERMEABLE GLOVES.

These impermeable films of India-rubber may be most strongly recommended both for the purposes of general practice, of accouchement, and of the dead-house. Instances are now almost beyond counting in which medical men have performed *post-mortem* examinations on subjects pregnant with infected poisons, and have themselves been infected and subsequently suffered severely, and in many cases fatally, from pyæmia. Likewise, in the course of accouchements, many a practitioner during vaginal examination has become infected and suffered permanently for the rest of his life. It would be a very wise precaution if practitioners should decide invariably to

use the impermeable glove when making *post-mortem* examinations, and in all doubtful cases of vaginal examinations. The makers of these gloves are Cow Hill and Co., of Cheapside. Although impermeable, as their name implies, the gloves are of a filmy thinness, so that they do not interfere with the delicacy of touch, and their use is, we believe, compatible with a very delicate perception of surface.—*London Medical Record*, May 15, 1879.

93.—STRUTT'S ABSORBENT WOOL.

Messrs. W. G. and J. Strutt, of Belper, have forwarded to us a specimen of cotton-wool prepared in a manner which will at once commend it for general medical and surgical uses as far superior to the ordinary cotton-wool now in use. Valuable as this is, by reason of its softness, elasticity, and relative absorbent power, it is far inferior to the specimen before us, which by some new process has been rendered so absolutely pure and free from grease of any kind, while it possesses a power of so rapidly absorbing moisture, that if it be thrown on water it at once sinks instead of floating, as does ordinary cotton-wool. The advantages of a cotton-wool which is thus pure and strongly absorbent will immediately present themselves to the minds of all medical and surgical practitioners; and this new material will, we believe, rapidly attain extensive popularity.—*British Medical Journal*, Aug. 30.

94.—THE MATLOCK INVALID COUCH AND MATLOCK BED-REST.

These excellent contrivances promise to add largely to the comfort of the acutely sick, the invalid, and the convalescent. They include an adaptation of the Excelsior flattened spiral springs, which long experience has shown to be the perfection of comfort and ease when applied to beds; and thus applied, in suitable framework, they form an invalid-couch which is unexceptionable as an article of furniture, whether as couch or settee, and which affords to the invalid a seat or lounge adjustable at a variety of angles and in a multiplicity of positions—light, easy, yielding, but perfectly elastic. As a bed-rest, it has similar advantages; and, whether for hospital use or in private houses, it is easy to predict an enduring popularity for these inventions. The Matlock Invalid Couch and the Matlock Bed-rest are manufactured by Chorlton and Dugdale, 19, Blackfriars Street, Manchester, who are the proprietors of the Excelsior spring mattress.—*British Medical Journal*, July 11.

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